Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

4 AUG 89 10.02
-R.L. GARWIN-

July 31, 1989

To: Cold Fusion Panel

The attached communications between Dr. Garwin and Dr. Menlove and Dr. Huizenga and Dr. Bockris are circulated for your information. Also enclosed is a letter from Garwin to me regarding a Pugwash meeting.

Bill

William L. Woodard
Secretary
Cold Fusion Panel

Enclosures

UNIVERSITY OF
ROCHESTER

COLLEGE OF ARTS AND SCIENCE
DEPARTMENT OF CHEMISTRY

John R. Huizenga
Tracy H. Harris Professor

July 25, 1989

Professor J. O'M. Bockris
Department of Chemistry
Texas A & M University
College Station, Texas 77843-3255

Dear Professor Bockris:

In response to your letter of July 17, 1989, I am sending you a copy of the DOE Panel's Interim Report, in case you haven't already seen it. I did not see the account in the New York Herald Tribune and, hence, cannot comment on it.

As you can see by the Panel's interim recommendations, more experiments need be designed to check the large amounts of tritium that you and your colleagues have reported. When possible, it would be desirable to measure also the neutron production in the same cells producing tritium. What is the present status of Storm's reported tritium? I have heard that he was working in a laboratory in Los Alamos where high-level tritium experiments were performed in the past.

The Panel is interested in learning about any new information you have on tritium production.

Sincerely

John R Huizenga
John R. Huizenga

Enclosure:

467 Hutchison Hall
University of Rochester
Rochester, New York 14627
(716) 275-4217

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July 17, 1989

Professor John Huizenga
University of Rochester
Dept. of Nuclear Chemistry
Rochester, NY 14627

Dear Professor Huizenga:

I have been away at a meeting in Spain for the last week, but on Thursday, in Madrid, I read an account in the New York Herald Tribune to the effect that the DOE committee on Cold Fusion had come to the conclusion that it was not necessary to build new laboratories and spend $25M in the study of the phenomenon, but that bursts of heat of an as yet unknown origin were being produced, and these are not due to errors of calorimetry.

I would like to point out that the most essential part - the implications of the production of tritium as a result of the electrolysis of deuterium oxide containing lithium deuteride - has not been featured. Large scale tritium production has been reported from five laboratories at present, including Los Alamos.

The bursts of heat have not been correlated with the production of tritium, and the phenomenon is still burdened with a lack or reproducibility but in any final report it is incumbent upon your committee either to give reason why they reject the validity of the dectection of large amounts ot T, or agree that sustained fusion is occurring. Our own tritium measurements have been verified on the liquids supplied by us at five institutions outside Texas A&M.

Even were the correlation between the tritium and the heat to be established, and reproducibility attained, there is no evidence to indicate that a great new technological pathway to energy has been founded. However, there is also no reason to believe that it has not.

As to the order of magnitude for the national budget in cold fusion, I am sure that this would need careful committee consideration (the committee consisting, I suppose, at least half scientists versed in physical electrochemistry), but I suggest that the early budget would be in the millions, and not in the tens of millions, per year, for the first few years (1%, say, of the present hot fusion budget).

Sincerely,

J. O'M. Bockris

JOMB/nm

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

July 24, 1989
(Via Airborne Express)

Dr. William L. Woodard
Cold Fusion Panel Secretary
Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, DC 20585

Dear Bill,

You have my letter of 07/12/89 to H.O. Menlove. Not having had a reply from him, I called several times, and finally made contact 07/17/89. This is a brief recording of his comments.

0. HOM: We have not separated the counters, because it is inconvenient. We will do something like that if we can't get a sample to "speak up on demand." But two or three of the main noise sources bypass that control-- noise from fluorescent lamps, line noise, and cosmic ray bursts.

1. HOM: We can alternate sample and dummy. In fact, about half as many dummy samples as real ones are run. We have never gotten a signal from a dummy. (I believe this means they have run about 14 real samples and 6 dummies.)

2. HOM: We should measure the 760 keV peak on a multi-channel analyzer.

3. HOM: We measure the time distribution of the neutrons within a burst and show that it is the same as the die-away time. The ratio of counts in the 128 microsecond gate to the number of counts in a 16 microsecond gate is 3 to 1. This is the same as detected from a Cf source. (In the Cf source, there is a fission burst of two or three neutrons. The first is used to trigger the apparatus, and the arrival of the second count is indicative of the time for emergence of a neutron from the moderator.)

4. HOM: One can run two or more systems in parallel.

To: W.L. Woodard, DOE (Via FAX to 9-(202)-586-3119).

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

July 12, 1989
(Via FAX to 9-(505)-667-7626)

Dr. Howard O. Menlove
Los Alamos National Laboratory
P.O. Box 1663, MS E-540
Los Alamos, NM 87545

Dear Howard,

You have probably received the message that I called from the ERAB Cold Nuclear Fusion Advisory Panel meeting in Washington 07/11/89. Here is what I wanted to ask, and I would be grateful for your reply by FAX, if possible.

Referring to your "Measurement of Neutron Emission ..." submitted to Nature 06/12/89, that we had at our Advisory Panel meeting in Washington 06/22/89 (LA-UR 89-1974), we find on page 3.9 (9/10 of the way down page 3) "The five ... pods on the circumference of System 3 can be used to determine the average neutron energy when sufficiently high neutron yields are obtained."

You may recall that I emphasized that the neutron "bursts" from Frascati could hardly be believed unless they produced counts simultaneously in a similar counter similarly disposed with respect to the source, whenever they produced massive numbers of counts in one counter. I urged also that LANL separate the counters in one of the detectors in order to ensure that electronically independent systems registered at the same time, coupled only by neutrons.

On reading further in your LA-UR 89-1974, I find on page 10.6, "We have not yet measured the neutron energies because the yields still are too low for the neutron spectrometry measurements." But I see in your Figure 4 149 counts on 05/21/89 in "System 3." What is the probability that 149 counts would be obtained in the inner counter tubes and none in the five outer tubes? In fact, if you have a record that the five outer tubes did not count in that 128 musec, this casts grave doubts on the identification of the burst as neutrons.

So please tell me whether you have made runs in which the counts of the outer five counters in System 3 can be correlated in time with the counts of the inner counters.

This is of particular importance because I have just read the Sandia paper "Search for Cold Fusion in High Pressure ...", by J.E. Schirber, et al, and I find on page 5.4 a very interesting paragraph,

> "We should point out however, that apparent neutron bursts have been observed a number of times in single counter channels ... Only those events which are observed simultaneously in our three counters with the correct relative intensities can be considered as arising from real bursts."

And on page 5.9,

> "It should be stressed that without n-counter redundancy, reports of the irreproducible generation of neutrons, including bursts, should be treated with great skepticism."

So now I have to ask for your data showing simultaneous counts in redundant counters.

Furthermore, I have to ask for your data showing counts in one counter set and no counts in the counters that might be expected to be influenced by the neutron bursts. These data should be presented, whether they fit your interpretation or not.

Finally, on re-reading your LA-UR 89-1974, I note that it had no experiments with pure hydrogen on metal, but only a 50:50 mixture. I don't recall seeing in the weekly Update from LANL any pure-hydrogen experiments. Have you performed them? What are the data?

Please reply to me by FAX to (914) 945-2141 with a copy to Bill Woodard at (202) 586-3119.

Thanks very much.

Sincerely yours,

/s/

Richard L. Garwin

cc:
W.L. Woodard, DOE (Via FAX to 9-(202)-586-3119).

RLG:jtml:193%HOM:071289.HOM

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

July 27, 1989
(Via FAX to 9-(202) 586-3119)

Dr. William L. Woodard
Cold Fusion Panel Secretary
Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, DC 20585

Dear Bill,

At a Pugwash meeting, 07/27/89, I met:

Dr. Ugo Valbusa
Dipartimento di Fisica
Via Dodecaneso 33
16136 Genova
ITALY

Dear Dr. Valbusa:
W: (39-10) 5993; x261 Office;
x246 Secretary; x356 Lab.

Valbusa is a solid state physicist who has been working with nuclear physicists in order to investigate cold nuclear fusion. At first they obtained seemingly positive results from D2 + Ti, using a system in which plastic scintillator was interspersed with Cd foil. Phototubes looking at the collection would see initially from a fast neutron the proton recoil, and then tens of microseconds later, another pulse which would be from the capture gamma rays in the Cd.

Small containers of 2-3 cc volume were immersed in the plastic scintillator, and counts were obtained on warming from nitrogen temperature.

A "blank experiment" was performed, with H2 instead of D2, and no counts were seen!

They were about to publish.

Then they tried empty containers, and they saw counts again.

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

They have discovered that if one puts a thermal gradient across the plastic scintillator, one gets counts!

They have now modified the system to use liquid scintillator instead of plastic, and a vacuum to isolate the Ti cell from the scintillator. This experiment will start soon.

In another experiment, they used Indium foil around the cell, inside a moderator. The idea was that neutron emission would activate the Indium, which could be counted in an ordinary counter later.

This is not so sensitive an experiment, but there is no possibility of confusion with thermal gradients, and the like. Their result is that they see fewer than 50 neutrons per second. That is, they see no neutrons above background, and their sensitivity is 50 neutrons per second.

Interestingly enough, Valbusa gave me word of the group at Cassacia, on which I have reported previously. Their experiment was to degas Ti at 1000 C, then to cool it to room temperature or to 500 C, and to contact it with 20 torr of D2. As you recall, they saw declining numbers of counts, of the order of 700 counts per minute for 5 minutes or 3000 counts per minute for 5 minutes in different trials.

However, the Cassacia group has now discovered that the problem is due to a difficulty in their BF3 counter! They have no positive result.

Science marches on!

Very best regards.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

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Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
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27 JUL 89 14.05
-R.L. GARWIN-

July 25, 1989

To: Cold Fusion Panel

The enclosed materials are circulated for your information.

Bill
William L. Woodard
Panel Secretary

Enclosures

072589.WLW

Search for Energetic Charged Particle Emission from Deuterated Ti and Pd Foils

P.B. Price, S.W. Barwick and W.T. Williams
Physics Department, University of California, Berkeley, CA 94720

and

J.D. Porter
Department of Chemistry, University of California, Berkeley
and Lawrence Berkeley Laboratory, Berkeley, CA 94720

Abstract

Searching for evidence of "cold" nuclear fusion in deuterium-loaded Ti and Pd foils with plastic *track-detectors*, we detected the emission of alpha particles from trace heavy element decay, but found no evidence of dd fusion. Cycling TiD_2 and $PdD_{>0.4}$ in high-pressure D_2 cells between 1 - 15 bar and 77 - 300 K, gave an upper bound of 0.7 cm^{-3} s^{-1} for the mean rate of dd → $^3He + n$ fusion. For electrolytically deuterated $PdD_{0.8}$ our upper bound is 0.0018 cm^{-3} s^{-1} for the mean rate of dd → p + t. This is ~1.5×10^7 and 180 times lower than "cold" fusion rates reported by Fleischmann *et al.* and Jones *et al.* respectively.

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Date: 1989.07.14

Version: 1989.07.14 (Fri) 18:08
Sourcefile: SEARCH2.MS

Our work was motivated by recent reports[1-6] of neutron emission from deuterated palladium and titanium. Those results have been interpreted as positive evidence for "cold" fusion of deuterons in these materials. To complement those experiments, we have searched for the recoiling charged particles which would also be produced by "cold" dd fusion reactions.

Two types of experiments have yielded positive evidence of excess neutrons in nearby detectors. In the first, electrolysis of heavy water electrolyte solutions is used to load Pd or Ti with deuterium by diffusion. From the neutron flux (BF_3 detector) reported by Fleischmann *et al.*[1] we calculate a specific rate for the neutron-producing dd fusion channel in their PdD_x of 2700 cm^{-3} s^{-1}; their reported excess heat outputs imply vastly higher fusion rates, $>10^{12}$ cm^{-3} s^{-1}. From the results of Jones *et al.*[2] we calculate 0.33 cm^{-3} s^{-1} for the neutron channel based upon their observations of 2.5 MeV neutrons in scintillation detectors near their PdD_x and TiD_2 cathodes. Wolf *et al.*[3] report neutron source strengths of 0.8 s^{-1} for PdD_x cathodes of unspecified volume.

In the second type of experiment, high-pressure D_2 gas is used to drive deuterons into Ti and Pd samples, which are then subjected to thermal and/or pressure cycling. De Ninno *et al.*[4] detected bursts of neutrons using a BF_3 counter during such sample cycling. Average source strengths during two separate episodes of activity were 20 n cm^{-3} s^{-1} and 260 n cm^{-3} s^{-1}. Ricci[5] reported a source strength of 10^6 n cm^{-3} s^{-1} for one ten minute episode. Menlove *et al.*[6], using ^3He neutron detectors, reported infrequent, low-level (~100) neutron bursts and random emission levels between 0.001-0.04 n cm^{-3} s^{-1} for sample mixtures containing Ti, Pd, V and Sn.

In our experiments, we used CR-39 plastic film (Track Analysis Systems Inc., Bristol, U.K.) to detect energetic charged particles. CR-39 detects products arising from both conventional fusion channels: $d + d \rightarrow p(3.02\ MeV) + t(1.01\ MeV)$, and $d + d \rightarrow {}^3He(0.82\ MeV) + n(2.45\ MeV)$. Damage tracks produced in CR-39 by ionizing particles are chemically etched (6.25 M NaOH, 70 °C) at a rate $v_T > v_G$, where v_G is the etch rate of the undamaged plastic. The resulting conical etchpits of cone angle $\Theta = \mathrm{arccsc}(s)$, where $s = (v_T/v_G)$, are identified and counted using optical scanning methods.

A particle's velocity βc and charge Z can be determined from its range R and from s, which is a function of the ratio (Z/β) (ref. 7). Detection efficiency is 100% for charged particles, including protons[8], of energy ≥0.2 MeV under ambient laboratory conditions.

In the present application, plastic track-detectors such as CR-39 offer several advantages over solid-state Silicon Surface Barrier (SSB) detectors like those used by Ziegler *et al.*[9] a) Electronic noise in SSBs limits particle detection to energies >1 MeV at low count rates. b) SSB background counts between 1-3 MeV, $\sim 2.5\ \mathrm{cm^{-2}\ d^{-1}}$, are >25x the integral background rate in CR-39. c) Burst emissions of particles may not be detected by SSBs because of their finite response time (>5μs) and limited energy window (1-8 MeV). d) CR-39 determines particle charge, energy, *location and propagation direction*, whereas single element SSBs simply measure the integral energy deposited in the active volume of the detector within its response time.

There are three types of charged particle backgrounds in CR-39 which are relevant to the present experiments. Between the time it is made and the time it is used, CR-39 accumulates cosmic ray spallation tracks and alpha decay tracks due to airborne radon. In our laboratory, the rate of production of detectable spallation recoil tracks is $\sim 1\ \mathrm{cm^{-2}\ yr^{-1}}$ (ref. 10), and radon alpha tracks accumulate at a rate $\sim 1\ \mathrm{cm^{-2}\ d^{-1}}$ while the film is exposed to open air. We were able to reduce these two sources of background to an equivalent accumulation rate of $<0.1\ \mathrm{cm^{-2}\ d^{-1}}$ by pre-etching the CR-39 for 8 hr and locating existing tracks before starting the present experiments, and by protecting the CR-39 from airborne radon during the experiments.

The third source of background tracks in the present experiments is alpha-emitting nuclides, predominantly the ^{238}U and ^{232}Th series, present as trace impurities in the Pd and Ti at ppm levels. Alpha particles emerge from the surfaces of the Pd and Ti foils with a continuous distribution of energies, from 0 to 8.8 MeV in this case, because their ranges are less than the thickness of the foils. In a control experiment, we determined the alpha track production rate of our Pd and Ti foils due to this source by exposing them to CR-39 for 41 days. Etching and analysis as described above yielded a production rate for ≥0.2 MeV alpha tracks of ~5 cm^{-2} d^{-1} for the Pd foil and ~1 cm^{-2} d^{-1} for the Ti foil. Note that alpha tracks do not interfere with a search for recoiling protons or tritons in CR-39, but those with lowest energy do place constraints on the determination of ^3He production rates.

Figure 1 shows the results of a calibration of the CR-39 film as well as the data for the two experiments discussed below. The curve labelled "alpha" was obtained by irradiating CR-39 with alpha particles, slowed to various energies, from a ^{252}Cf source. The curves labelled "tritons" and "protons" were scaled from the alpha particle curve using a restricted-energy-loss model of track formation[7]. The proton curve is quantitatively similar to that obtained for CR-39 neutron spectrometers whose basis of operation is the detection of recoiling protons[8].

Assuming that particles are emitted isotropically and uniformly throughout the samples, then the product of detection efficiency and effective source thickness, $<\eta H>$, can be calculated by integrating over emission zenith angle, Θ, and depth, z, measured from the CR-39 into the source:

(1) $$<\eta H> = (1/2) \int_0^{z_{max}} \int_0^{\Theta_{max}} \sin\Theta \; d\Theta \; dz$$

where Θ_{max} is the smaller of $\Theta_{max} = \arccos(z/z_{max})$ or $\Theta_{max} = \arccos(v_G/v_T)$ and z_{max} is the smaller of $z_{max} = d_s$, where d_s is the sample thickness, or $z_{max} = (R-\delta R)-\Sigma(d_i)$. R is the particle's range in the material, δR is the minimum range needed for detection in the CR-39, and $\Sigma(d_i)$ is the sum of the density-corrected thicknesses of all materials between the sample and CR-39. The dd fusion products are calculated to have the following ranges in PdD_x: R(p) = 33.0 µm, R(t) = 4.6 µm and $R(^3He)$ = 1.3 µm. As an example, for 0.82 MeV ^3He produced from dd fusion and detected in CR-39, $\arccos(v_G/v_T) \sim 80°$, $<\eta H> \sim 0.5$ µm in $PdD_{0.6}$ and $<\eta H> \sim 0.8$ µm in TiD_2.

For our first experiment we cleaned a 0.1-mm-thick Ti foil (purity >99.6%) and a 0.23-mm-thick Pd foil (unknown purity) using *aqua regia*. After heating the foils to 550 °C for 3 hr in

D_2 gas at 1 bar they were allowed to cool in D_2 to room temperature overnight. The diffusion coefficient for D in Pd at room temperature is 5×10^{-7} cm^2 s^{-1} (ref. 11), high enough to fill the foil in a few hours, but temperatures ~500 °C are required to achieve the same diffusion coefficient in Ti (ref. 12). Final stoichiometries of the deuterated foils were determined by weight gain to be TiD_2 and $PdD_{>0.4}$, the value for Pd being a lower bound because of possible loss of deuterium dring handling. These values are consistent with known phase diagrams[13,14].

The deuterated foils were then clamped against CR-39 sheets (contact area 2 cm^2 for each sample) and put inside a stainless steel pressure cell attached to a source of D_2 gas. After evacuating the cell using a roughing pump, we loaded D_2 gas to a pressure of 15 bar and immersed the cell in liquid nitrogen for 1 hr to allow sufficient time to cool the chamber, and then removed it from the liquid nitrogen, allowing it to warm to 20 °C in about 2 hr. The cooling and warming cycle was repeated two more times. Prior to the last warming we removed the D_2 gas from the chamber with a roughing pump. The entire experiment lasted about 9 hr, after which we etched the CR-39 films for 1.7 hr and searched the regions that had been in contact with the Pd and Ti foils for tracks produced since the pre-existing ones were located.

Two tracks of particles with Z = 2 came from the Pd (labelled × in Figure 1), and no tracks came from the Ti. We calibrated the response of CR-39 in the high-pressure cell at 15 bar and 77 K using alpha particles from ^{238}U decay. We found that the sensitivity to an isotropic flux of particles with Z = 2 inside the cell at 77 K was ~80% of that at 20 °C and 1 bar. The energies of both Z = 2 particles from the deuterated Pd foil were too high to be due to 0.82 MeV ^{3}He. Track production rate is consistent with the alpha particle emission rate for the Pd foil determined in the control experiment. This null result was converted into an upper limit for the rate of the dd → ^{3}He + n channel using equation 1, and is compared in Table 1 with the dd fusion rates inferred from the neutron fluxes reported by De Ninno *et al.*, Ricci and Menlove *et al.* Lacking a source of particles with Z = 1 for a calibration inside the high-pressure cell, we report results only for the n + ^{3}He product channel.

Figure 2 shows an "exploded" view of the electrochemical cells used in our second experiment. The setup was designed so that the light water control cell matched the heavy water cell as closely as possible. Both cathodes were cut from the same piece of 25-μm-thick Pd sheet (99.9%), and had a 100 nm Au diffusion barrier evaporated on the side which faced the CR-39. The cells were operated in electrical series using a single current source (Keithley 220). Viton O-rings defined 1.26 cm^2 active electrode areas. Solutions were prepared by reactir [illegible] egreased, isopropanol-etched Li under nitrogen with either 18 Mohm-cm H_2O (Milli-Q Plus purified, Anal. 0.014% D, <2 dpm mL^{-1} T) to form 1.00 M LiOH, or with D_2O (Cambridge Isotopes Laboratories, Lot# F7962, Anal. 0.13% H, 17.9 dpm mL^{-1} T) to form 1.00 M LiOD.

The electrodes were presaturated with either H or D by constant current electrolysis at 10 mA cm^{-2} for 12 hours. Coulometry during the presaturation period indicated initial electrode stoichiometries of $PdH_{0.65}$ and $PdD_{0.74}$ at the point of first gas bubble formation. The cells were then left at open circuit for 10 hr. Finally, electrolysis was performed at 1.00 mA cm^{-2} for 13 days. Steady-state cell operating voltages (includes ~0.03-0.05 V iR-drop component and kinetic overpotential) were 2.77 V (H_2O) and 2.96 V (D_2O). Cathode stoichiometries were determined by mass change to be $Pd(H,D)_{0.8}$, a value which is consistent with *in situ* EXAFS measurements of Pd foil cathodes[15]. This stoichiometry would be achieved in gas cells operating at ~70 bar at room temperature[13].

After the run we etched the CR-39 for 8 hrs and searched for new tracks in the circular areas that had been in contact with the cathodes. We found 73 tracks at the light water location and 65 tracks in the heavy water location; all tracks were due to particles with $Z = 2$. The data are shown in Figure 1. The track production rates agree at the 1σ level and are consistent with the alpha particle emission rate for the native Pd foil discussed above. There were no tracks due to protons with $0.2 \text{ MeV} \leq E \leq 3 \text{ MeV}$ or tritons with $0.2 \text{ MeV} \leq E \leq 1 \text{ MeV}$.

For 3 MeV protons from dd fusion, $\arccos(v_G/v_T) \sim 45°$ in CR-39, and equation 1 gives $<\eta H>$

$\approx$ 3.9 μm for a 26.3-μm-thick $PdD_{0.8}$ source. Similarly, for 1 MeV tritons with $\arccos(v_G/v_T) \sim 60°$, equation 1 gives $<\eta H> \approx 0.63$ μm for a 26.3-μm-thick $PdD_{0.8}$ source. Table 1 lists our two upper limits on the dd $\rightarrow$ p + t channel. We compare this limit with the result of Ziegler *et al.*[9] as follows: Using equation 1 we get $\approx$ 3.1 μm for their conditions (Expt. 1) of 1 - 3 MeV protons, 25-μm-thick Pd cathode, 1.76-μm-thick gold diffusion barrier and 2 mm air gap. From their observed count rate of 40 *per* 205 hr we calculate the limiting fusion rate listed in Table 1. The background rate in their experiments was consistent with the rate we found for alpha emission from radioactive impurities in our Pd foil. The reasons why our limit on the dd fusion rate is lower than that of Ziegler *et al.* are: we have no detectable background for protons, we used a thinner gold diffusion barrier, there was no air gap, and CR-39 has a lower minimum detectable particle energy than SSBs.

In conclusion, the only energetic charged particles we detected in both our experiments were alpha particles emitted from trace radioactive impurities in Pd and Ti at rates of a few particles *per* cm^2 *per* day, independent of the deuterium content of the foils. Our upper limits on average dd "cold" fusion rates are far below those inferred from neutron detection in experiments involving either electrolytic[1,2] or high-pressure[6] loading of D_2 into Pd or Ti. To rationalize our null results with claims of episodic "cold" dd fusion[3-6], it would be necessary to argue that fusion at the average rate reported by Jones *et al.*[2] occurred less than 1/180 of the time during which our electrolytic cell was operating and that fusion at the rate reported by De Ninno *et al.* occurred less than 1/400 of the time during which we cycled TiD_2 in D_2 gas. It is exactly these arguments which have been put forth by Menlove *et al.*[8] to explain their low neutron emission rates, which still match or exceed our detection limits. Our limits apply to any mechanism that produces energetic charged particles in deuterated Pd or Ti, such as the neutron-initiated chain reaction proposed by Jackson[16].

This work was supported *in* part by NSF Grant PHY-8702763 and by the Director, Office of Energy Research of the U.S. Department of Energy under Contract No. DE-AC03-76SF00098.

References

1. M. Fleischmann, S. Pons and M. Hawkins, J. Electroanal. Chem., 261, 301 (1989)
2. S.E. Jones *et al.*, Nature 338, 737 (1989)
3. K.L. Wolf *et al.*, Workshop on Cold Fusion Phenomena, Santa Fe, NM, May 23-25 1989.
4. A. De Ninno *et al.*, submitted to Europhys. Lett. (1989)
5. R.A. Ricci, Fusion Workshop, Varenna, Italy, May 2-4, 1989
6. H.O. Menlove *et al.*, submitted to Nature (1989)
7. R.L. Fleischer, P.B. Price and R.M. Walker, "Nuclear Tracks in Solids", University of California Press, Berkeley, 1975.
8. T.W. Turner, D.L. Henshaw and A.P. Fews, Nuclear Tracks and Rdn. Meas. 8, 341 (1984)
9. J.F. Ziegler *et al.*, Phys. Rev. Lett. 62(25), 2929 (1989)
10. P.B. Price, unpublished results (1982)
11. J. Voelkl *et al.*, Z. Naturforsch. 26a, 922 (1971)
12. R.J. Wasilewski and G.L. Kehl, Metallurgia 50, 225 (1954)
13. R.P. Santandrea and R.G. Behrens, "High Temperature Materials and Processes", Vol. 7, Nos. 2 & 3, pp.149-169 (1986).
14. M. Arita *et al.*, Met. Transactions 13A, 1329 (1982)
15. R. Frahm *et al.*, Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 23-25, 1989.
16. J.C. Jackson, Nature 339, 345 (1989)

TABLE I. "Cold" Fusion Rates Inferred from Measurements of Energetic Particles

	Sample	**Duration** (h)	**Particle** (Detector)	**Fusion Rate** (cm^{-3} s^{-1})^{a}
Electrolysis Experiments				
Fleischmann *et al.* (ref. 1)	PdD_x	50	neutrons (BF_3)	2700
Jones *et al.* (ref. 2)	TiD_2 (#6)	8	neutrons (scint.)	0.33
Wolf *et al.* (ref. 3)	PdD_x	1-2	neutrons (NE213)	($0.8/V_{Pd}$)
Ziegler *et al.* (ref. 9)	$PdD_{>0.6}$	250	protons (SSB)	<0.028^{b}
This work	$PdD_{0.8}$	307	protons (CR-39)	<0.0018
	$PdD_{0.8}$	307	tritons (CR-39)	<0.011
	$PdD_{0.8}$	307	p or t (CR-39)	<0.002
High-Pressure D_2 Gas Experiments				
Menlove *et al.* (ref. 6)	"Ti-1"	18	neutrons (^{3}He)	0.001- 0.04
De Ninno *et al.* (ref. 4)	TiD_x	13^{c}	neutrons (BF_3)	~20
	TiD_x	40^{c}	neutrons (BF_3)	~260
Ricci *et al.* (ref. 5)	PdD_x	0.17^{c}	neutrons	up to 10^{6}
This work	TiD_2	9	^{3}He (CR-39)	<0.73
	$PdD_{>0.4}$	9	^{3}He (CR-39)	<2.6

---

a. To convert to fusions *per* dd pair *per* second: For TiD_2, $1\ cm^{-3}\ s^{-1} = 4.72 \times 10^{22}\ (dd)^{-1}\ s^{-1}$
For PdD_x, $1\ cm^{-3}\ s^{-1} = [2x/(0.3925+0.0165x)^3] \times 10^{21}\ (dd)^{-1}\ s^{-1}$

b. This takes into account detector solid angle collection factor, the 1-3 MeV energy window and the ranges of charged particles in the Pd cathode + gold film + air gap for the first expt.

c. Period during which excess neutrons were detected.

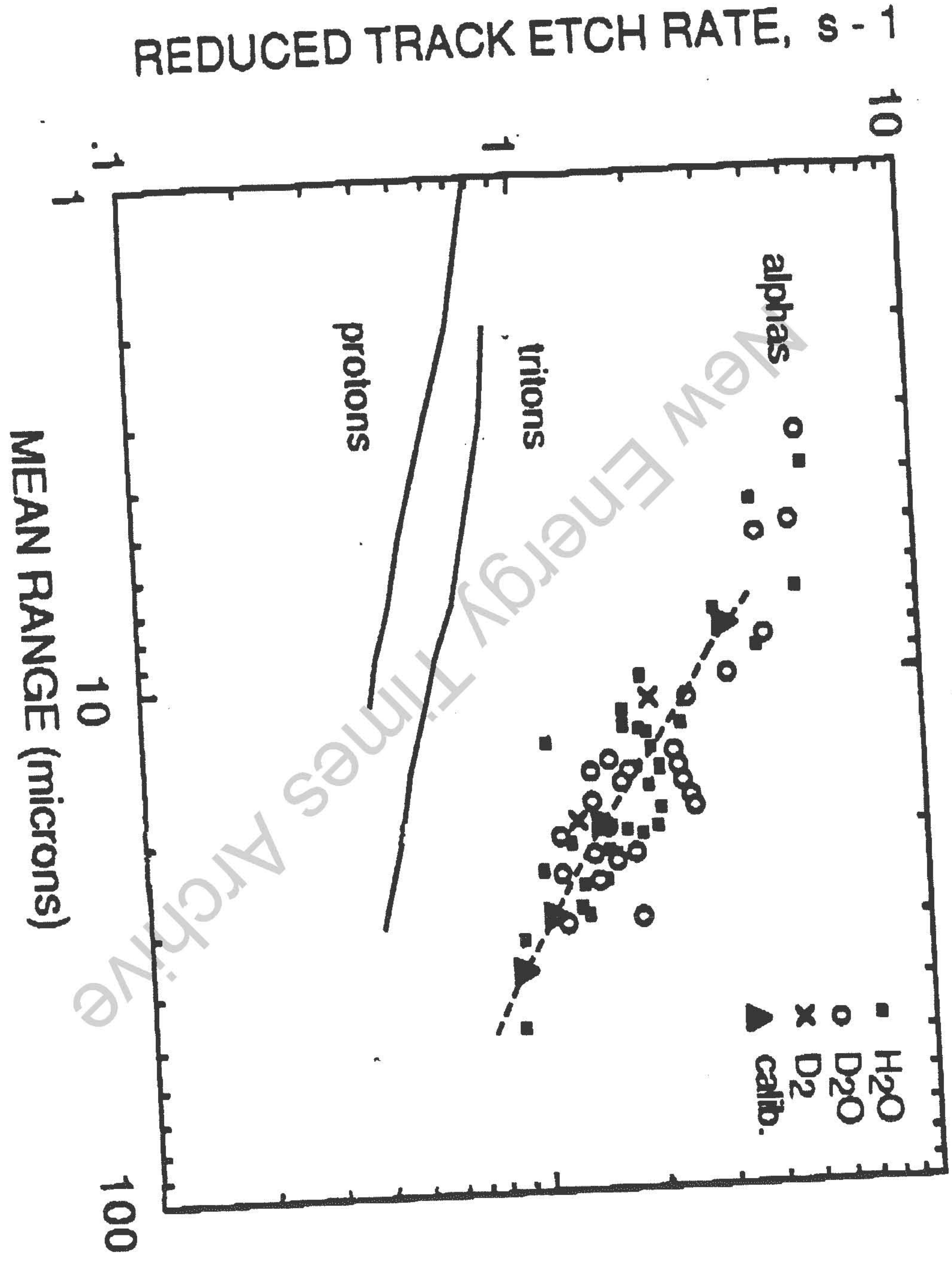
Figure Legends

Figure 1.

Energetic particles detected by CR-39 film next to Pd foils: (■) Electrolysis cell, H_2O, 1 M LiOH, (O) Electrolysis cell, D_2O, 1 M LiOD, (×) High-pressure D_2 gas cell. (- ▲ -) Calibration with ^4He particles from ^{252}Cf.

Figure 2.

"Exploded" schematic of the twin electrochemical cells. Cell bodies were 1.00 cm lengths of plexiglas tubing with a 2 mm fill/vent hole and grooves machined for the viton O-rings. Anodes were 2 μm e-beam evaporated Pd on 100 nm Au on microscope slides. The whole assembly was held in a small vise.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10
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MEAN RANGE (microns)
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Figure 1.

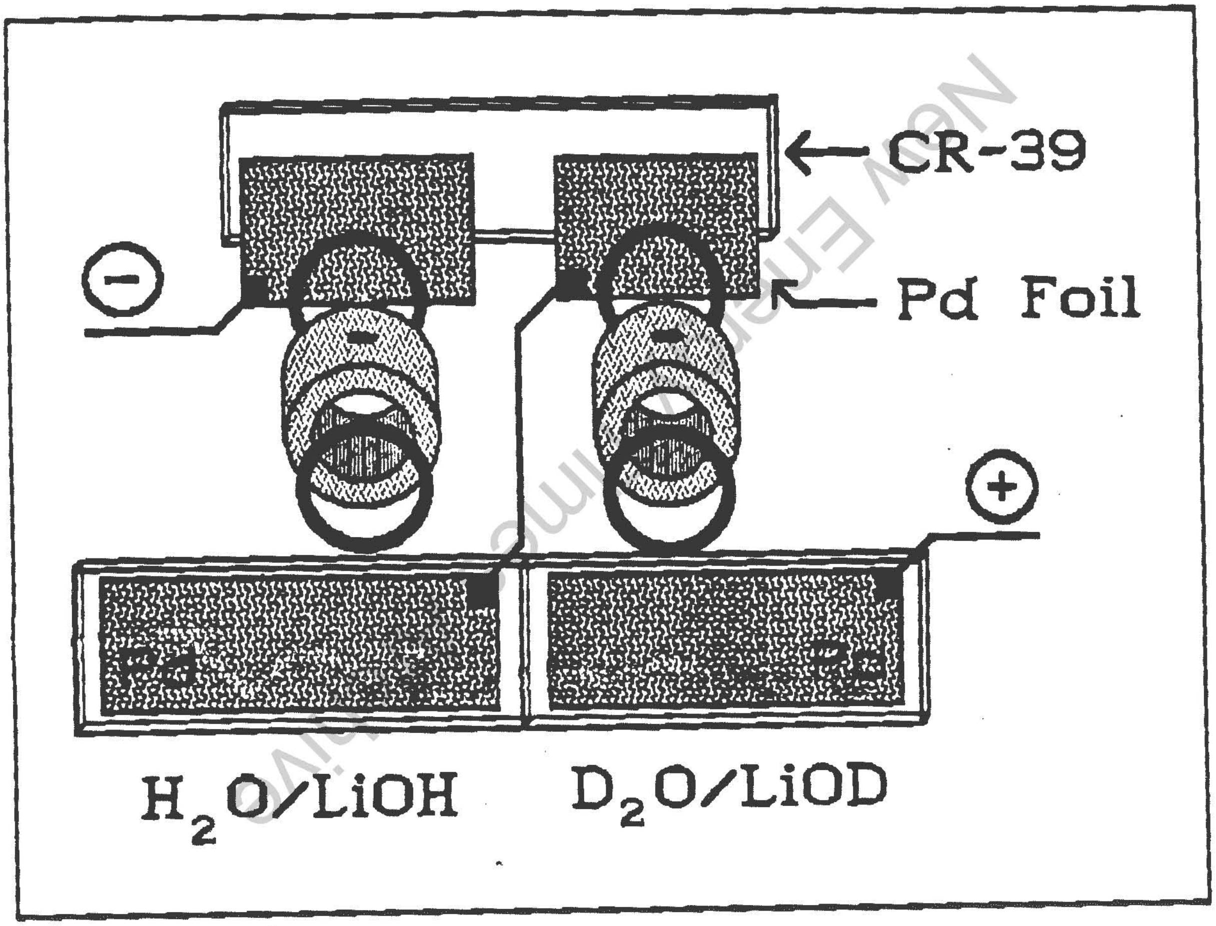

Figure 2.

Sent to
Huizenga
via FAX
John! Have you
seen this?

SUMMARY OF COLD FUSION RESEARCH PROGRAM
Sandia National Laboratories
June 15, 1989

I. OVERVIEW

Since the announcement of cold fusion in late March by the University of Utah investigators we have carried out an extensive range of research activities at Sandia National Laboratories in an attempt to duplicate those experiments and also to gain evidence for the existence of this phenomenon. The program has focused on experiments of both the University of Utah type, where significant excess heat was reported, and on the Brigham Young University type, where a lower level of fusion reaction was reported. Approximately twenty members of technical staff have been involved, representing expertise in materials science, radiation physics, electrochemistry, solid state physics and metallurgy. We find no evidence for the existence of cold fusion. Remaining investigations are focused on our sensitive calorimetry experiment to search for the production of excess heat at longer operating times.

backg 10 c/hr + eff. of 9.2%

II. DETECTION SYSTEMS

Three special detection capabilities have been utilized in these studies. First we have set up a multi-detector neutron detection system of ^{3}He proportional counters and located it underground on Kirtland Air Force Base near our Albuquerque location. This has allowed us to carry out studies with both electrochemical cells and high pressure gas cells with the capability of detecting continuous neutron emissions corresponding to production levels as low as 100 neutrons/hr or bursts as low as 50 neutrons. Not only have these underground experiments allowed us to reduce background levels by three orders of magnitude from aboveground tests, but by correlating the signals from three independent detectors operating simultaneously we have found that we are able to essentially eliminate spurious artifacts which occasionally give false signals in one detector system but not in all three simultaneously. Single-detector artifacts were observed that closely mimic both continuous and burst emissions reported previously as being due to cold fusion.

A second special capability is a closed cycle Freon calorimeter system in which we are performing electrochemical cell experiments. This calorimeter has the advantage of being sensitive to the presence of small amounts of excess heat at high power levels (e.g., 1% at 20 W). Also, the total energy input is monitored from initiation of the experiment. We are presently conducting experiments with palladium electrode volumes >0.8 cm^3 and current densities above 300 mA/cm^2, while taking into account all experimental details proposed to be important by Pons and Fleischmann and by Huggins, but with a much more sensitive and accurate calorimetry system.

Finally we have utilized sensitive charged particle detection in conjunction with thin (25 μm) Pd foils where the foil forms one wall of the electrochemical cell. This approach allows the t+p branch of the D-D fusion reaction to be detected with essentially no background at very large detection efficiencies. The above three detection approaches have allowed us to exceed the sensitivity of the U. of Utah and the BYU experiments.

III. COLD FUSION EXPERIMENTS

A. Electrochemical Cells—(1) U. of Utah of Type: Electrochemical cells designed after the Pons and Fleischmann experiment have been carried out using a wide variety of electrode preparations, size, current densities, and times (up to 45 days). We have searched for the presence of radiation using neutron detectors, tritium by analyzing the electrolyte, and excess heat with our sensitive calorimetry system. We feel these experiments require another 10 to 15 days to exclude the possibility of excess heat generation, since our sensitive calorimetry under large sample volumes and high current densities has only been in operation for about two weeks. (2) BYU Type: Electrochemical cell experiments of the type reported by Jones et al. have been conducted using a wide variety of electrodes including Ti and Pd, immersed in LiOD or the Jones "mother earth" electrolytes. These were carried out under normal laboratory conditions as well as at the high neutron sensitivity underground location. (3) Thin Wall Cell Type: We have also utilized our thin wall cell configuration to obtain an order of magnitude higher sensitivity than in the Jones experiment (for bulk reaction rates within the Pd thin foils). These studies, as well as those of the BYU type, have been carried out for both steady-state and pulsed charging modes to look for cold fusion under highly non-equilibrium conditions.

B. Pressure/Temperature Cycling—We have carried out a wide variety of experiments in which pressure and temperature have been cycled between 1 atm and 4 kbar and between room temperature and 77 K. Materials included Pd powder, Ti powder, sintered Pd-Ti powder of the prescription given by Jones, TiD$_2$, Ti electrodes soaked in Jones electrolyte, and TiV and Zr shavings. The sensitive underground neutron detection system was used.

C. Plasma Loading—We have conducted experiments in which the atomic deuterium was introduced into Pd powder by plasma loading and the radiation measured by neutron and charged particle detectors. Experiments focused on understanding conditions for minimizing surface recombination and allowed loading ratios of about 0.9 D-Pd to be obtained.

D. Implantation Loading—Direct deuterium ion implantation into Pd and Zr at low substrate temperatures (40 K) were carried out to achieve superstoichiometric conditions. In the case of Pd, for example, we achieved loading ratios of D/Pd =1.6, with charged particle detectors used to monitor the t+p branch of the cold fusion reaction.

E. Chemical Assays for Fusion Products—We collaborated with Rockwell International (Canoga Park, CA, 91303) in the search for evidence of ^{3}He or ^{4}He fusion products in Pd electrodes from electrochemical cells provided by a number of U.S. laboratories. Measurements at Rockwell's Helium Assay Facility involve vaporizing samples under vacuum, passing all gases through multiple getter stages and measuring the helium concentrations using a precision mass spectrometer that had been calibrated with Pd samples implanted at various ^{4}He fluences by Sandia. The ultimate detection level is ~1 x 10^{10} helium (^{3}He or ^{4}He) atoms per gram of Pd and in no case (including samples from Texas A&M that were reported to exhibit either excess heat, neutron or tritium production) was there any helium product (^{3}He or ^{4}He) detected above system background.

In all the above experiments we were unable to find evidence for the presence of any radiation, radiation products, or excess heat which would have indicated the presence of cold fusion. We believe that we have faithfully reproduced the experimental conditions as best known and have a greater sensitivity for the presence of the phenomenon than that reported by the original investigators.

IV. SUPPORTING STUDIES

A. Loading Kinetics of Electrochemical Cells—Two experiments were conducted to better understand the actual loading kinetics of Pd electrodes, since it has been speculated that high D-to-metal loading ratios are important and there have been questions as to the loading times, cell operating conditions and electrolyte conditions under which proper loading may be achieved. In the first experiments thin wall cells were utilized in conjunction with an external ion beam for direct in situ analysis of the D concentration within the Pd metal. Loading dynamics as well as D/H poisoning kinetics were examined. In the second set of experiments the permeation through electrochemical cell thin foils examined the influence of palladium electrode surface preparation on the loading achieved.

B. Deuterium Atom Spacing—Molecular dynamics and total energy band structure calculations were carried out to examine the minimum D-D spacings which would be expected to be achieved. Theoretical consideration was also given to possible closer spacings on the surface of Pd. Even for superstoichiometric D:Pd ratios no evidence for D-D spacings closer than that for the D_2 molecule (about 0.7 A) was found under the most favorable model conditions, and in all of these studies no evidence was found to predict a sufficiently close spacing as required by the proposed rates for cold fusion.

C. Power Balance Calculations—The assertion by Profs. Fleischmann and Pons of excess energy production is derived from a power balance calculation based ultimately on Newton's Law of Cooling which may not be directly applicable in their situation. They use a submerged resistor to heat the electrolyte solution and assume a constant heat transfer coefficient which in turn is used to calculate the energy loss rate, $k_T\Delta T$, to the surrounding environment. However, total heat loss (especially from evacuated Dewar cells) may be dominated by evaporation of electrolyte which increases exponentially with temperature. Calculations under the assumption of evaporation-dominated heat loss suggest that there is, in actuality, little or no excess energy produced.

V. SUMMARY

In summary we have investigated electrochemical loading, pressure and temperature cycling, and the direct formation of superstoichiometric metal hydrides by implantation for the production of cold fusion. We have found new ways to probe the kinetics of hydrogen isotope loading of electrochemical cells and we anticipate that these may bring research benefits outside cold fusion. We have carried out detailed neutron detection at extremely low rates underground with unexpected spurious continuous and burst signals in helium-3 detectors not previously appreciated. We find multiple detectors provide an excellent way to exclude spurious signals which might otherwise be interpreted as evidence for cold fusion. Finally, we find no evidence to support the existence of cold fusion in any of our studies or to suggest the appropriateness of initiating an extensive program of research in this area.

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July 18, 1989

Mr. William Woodard
Secretary, Cold Fusion Panel
ER-6
3F-043
US Department of Energy
1000 Independence Avenue SW
Washington, DC 20585

Dear Mr. Woodard :

I comment here upon your report on the Committee of Cold Fusion which was published on the 12th of July.

1. Throughout the report it has been assumed that the normal branching ratio is that of the fusion reactions which are discussed for plasmas and which produce on the one hand helium and neutrons and on the other hand tritium and protons remains intact in the interfacial deuteron transfer situation under fields which reach 10^{10} volts cm^{-1}.

This is a gratuitous assumption because of the entirely different situation which exists in cold fusion carried at the electrode surface compared with that which is carried out in a plasma. As a speculative hypothesis, for example, during the transition of a deuteron across the double layer at the electrode, the dipole moment of the neutron will cause rotation so that, upon reaching the surface, proton transfer reaction will be preferred. Thus, if tritium is produced, it is not necessarily reasonable to ask for the corresponding neutrons calculated upon fusion in the plasma state.

In general there should be a reliance upon experiments. Objections arising from the theories which were relevant to plasmas and other types of fusion should be kept in abeyance while valid experiments are performed. The only thing should be the validity of the experiments.

The discovery of high temps of superconductivity is sufficiently recent to recall that no such phenomena had been predicted by theorists who at the present time are still unable to produce a consistent and agreed to theory although there is no doubt about the phenomena (It should, -as with Cold Fusion) - be "impossible" according to theory.

Page 2 of the report a few lines from the bottom, state that no one has observed excess heat in closed cells. This is not true. McKubre at the Stanford Research Institute has observed 5 bursts of high heat (heat greater than the total electrical energy input) in a cell which was closed. In fact, McKubre's cell was probably the best cell of all, in a sense that he used a deuterium anode, dissolving deuterium to form deuterium ions and thus did not involve oxygen at all so that the entire idea of recombination as a source of heat would not enter the discussion.

Page 3 of the report at the top. Here the much discussed areas of calorimetry are touched upon and although one must evaluate each calorimeter perhaps in each laboratory I must say that as my experience grows I discount increasingly errors of calorimetry. We use quite simple calorimeters and about 3/4 of our results, - maybe even 4/5 of them, - most unhappily lie upon exactly the predictions of electrochemical theory. When the cell begins to produce heat, it produces heat in a most certain manner, deviating quite unambiguously from the line, sticking out from it, until it has finished its heat production whereupon it goes back exactly on the electrochemical line.

Page 3 - fusion products. In the third paragraph it is said, "If the tritium were a result of deuterium fusion, the rate of neutron production should be comparable and some 10^{10} times greater than that reported."

Now, in the experiments carried out at Texas A&M, it is possible to say that the production of tritium was "certain". I base this unusual statement on the fact that the liquids from these results were verified by 5 independent laboratories outside that at Texas A&M, including the national tritium center at Los Alamos. Although the tritium produced varied with time as the electrode run proceeded, rising in an S shaped curve to a steady state presumably corresponding to the saturation of the solution and the partial pressure of DT in the gas. Large amounts of T have also recently been found in the gas.

Similar results which have also been obtained at Los Alamos, the University of Florida and centers overseas.

Now, it is up to the panel, of course, to explain the method of producing tritium from deuterium which does not involve fusion. Personally, I cannot conceive a chemical way of converting one isotope to the other and it seems that the comments which are of the type "if so and so many tritiums are produced then so and so many neutrons would be produced" depends on the branching ratio assumption which, of course, may not be applicable to the solid state fusion.

On page 3 the last paragraph, the material here about heating also involves two assumptions which may not be applicable in solid state fusion.

On the one hand there is material about heating inside the electrode, but what if fusion has occurred on the surface (there is now plenty of evidence for this, - including the fact that one may clean the surface of the electrode and change the production rate of the neutrons). Correspondingly, these statements again assume a constant branching ratio. One sees once more the entry of the older theory, established under quite different additions, into the newer phenomenon.

Lastly, in respect to the recommendations I would have two comments.

A. I think it is essential to stress the need for physical electrochemists working on these phenomena. It is rather retrograde to have physicists not familiar with the electrochemical technique working on the phenomenon and as far as the types of electrochemistry concerned, those tending to be electroanalytical or solution side, or those tending to be physical or "inside the electrode kind", it is clearly the latter who are most relevant.

If and when we get consistent results the physicists will be most valuable in trying to interpret them, but the experimental work should be concentrated on the laboratories of physical electrochemistry.

Lastly, I would comment on what is meant by "modest level of support." I certainly am myself not one who would think, e.g., $25,000,000 support is justified at this time. We are just beginning the investigation of a new field.

Although there certainly has been too much hype, too much exaggeration and initially too many claims to this phenomenon; although its present status is doubtful, hesitant and irreproducible, there is also nothing to deny the original claim and hope, - a method of obtaining sustained fusion. For example, both Wadsworth and McKubre's results showed bursts of heat lasting several hours. What one needs is for these results to become reproducible and durable, on, say, titanium.

A proper assessment of the level of spending could be assessed by a committee of a few knowledgeable physical electrochemists, who have practical experience in the field. What one needs is a few (one thinks of about six) laboratories each with a team (one thinks of a dozen) persons that should be enough to concentrate on the right things and to exchange and check results.

If we think of four universities and two government laboratories, we are thinking about something in the region of $4,000,000 per year. By happenstance this happens to be 1% of the hot fusion budget.

Sincerely,

J. O'M. Bockris

StonyBrook

Department of Chemistry
State University of New York at Stony Brook
Stony Brook, New York 11794-3400
(516)-632-7905
BITNET: JBIGELEI@SBCCMAIL
FAX: 516-632-7960

18 July 1989

Mr. William Woodard,
ER-6, 3F-043
US Department of Energy,
1000 Independence Ave., SW,
Washington, D.C. 20585

Dear Bill,

I enclose copies of some recent correspondence I have received from Professor Bockris of Texas A&M along with my responses. Please add these to your files and distribute them via paper mail to the members of the ERAB Cold Fusion Panel.

Sinceely,

Jacob B.

Jacob Bigeleisen
Distinguished Professor

TEXAS A&M UNIVERSITY
DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

June 20, 1989

Dr. Jacob Bigeleisen
University of Rochester
Dept. of Chemistry
Rochester, NY

Dear Dr. Bigeleisen:

I am sending you herewith a fragment of a paper which is being written.

The fragement refers to the interjection which you made in my presentation at Texas A&M.

In essence I show a consistence between the hypothetical heat production of my electrodes while producing tritium and the concept that the tritium existed at the time of the measurement as DT in solution, and DT is in the gas phase as a fraction (~10^{-7}) of the D_2.

I say: Suppose T is formed via $D + D \rightarrow T + H$; and that 5 watts cc^{-1} is being produced from that source. Then, if the T is mostly in the gas phase (as DT mixed with D_2) and if some T (that which we measured) is in solution as DT, - then there is a reasonable and approximate tie-up of the final observed concentration of DT in solution after 10 hours at high current density.

Please comment,

Sincerely,

J. O'M. Bockris
Distinguished Professor of Chemistry

JOMB/nr

Two situations may give rise to the sporadicity on the surface. One thing is a change of mechanism. It is known that the mechanism of an evolution of hydrogen involv two extremes.

In the one, the θ, or a degree of occupancy, is far over towards the side of being "nearly full".

In other mechanisms, the θ_H on the surface is below 0.1.

However, which of the mechanisms pertain is again a matter of fine distinction. Traces of impurity on the surface can change the mechanism easily (and can change the equilibrium of the interior.)

The general characteristics therefore strongly hint towards a surface fusional reaction.

PART OF DRAFT
PAPER IN PREPARATION

CALCULATION OF TRITIUM IN SOLUTION

Thus, the high current density being used here is at 0.5 amps cm^{-2}, and as the electrode diameter is 3 mm, and the length 5 cm, the electrode area is about 4.2 cm, i.e., total current is 2.1 amps. The volts of the cell are about 6, so the electrochemical heat production is 12.6 watts. Of this, some 10-15%

is in excess so that we may say our cell is producing about 1.5 joules/second of excess heat.

you use here D+D   T+H, the production per molar act is 4.02 Mev, and this is $3.9.10^{11}$ joules.

The total number of moles of D_2 produced per second is given by

$$\frac{2.1}{2.10^5} - 10^{-5} \text{ moles } D_2 \text{ sec}^{-1}$$

Thus, if each act of D_2 formation also led to the production of DT, then the rate of heat production would be $10^{-5}.3.9.10^{11}\text{sec}^{-1}$ joules/sec, i.e., $3.9.10^6$ joules.

In fact, the heat is 1.5 joules per second, so that the fraction of the number of acts which produce tritium per second on this hypothesis is given by $1.5/3.9.10^6 - 3.8.10^{-7}$.

Over the solution, the total pressure of the gas is about 1 atmosphere, so that the actual pressure here of the hypothetical DT is $3.8.10^{-7}$ atmospheres.

If the total pressure of hydrogen at 1 atmosphere and concentration of H_2 in the solution would be about 2.10^{-6} moles/cc, making no corrections for isotopic differences (but Nigel Packham, you could do this). We expect the concentration in the solution to be

2.10^{-6} . 3.8 [illegible]$^{-7}$ moles/cc = $7.6.$[illegible]$^{-13}$ moles/cc = $4.5.10^{11}$ particles/cc

In fact, they have about 20.10^{11} particles/cc. In view of the approximations, it may be that it is quite good, i.e., we get reasonable order of magnitude agreement with the hypothesis that all the heat is being produced at the surface, and the final build-up of atoms in the solution is in equilibrium with the amount of DT which one would expect on the hypothesis given to be in the gas phase.

NEEDED ENERGY FOR FUSION

The fusion of nuclei occur when temperature of the plasma is about 10^{8}°K. Assuming that the energy is 3/2RT, this is about $1.3.10^{4}$ ev.

One may imagine that the shape of the relationship between fusion and energy is an S-shaped curve of the following kind:

(Nigel draw S-shaped curve in which we show as the

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July 5, 1989

Dr. Jacob Bigeleisen
State University of NY at Stony Brook
Chemistry Department
Stony Brook, NY 11794-3400

Dear Dr. Bigeleisen:

You will be interested to know that we have now taken up your suggestion of making a direct measurement of tritium in the gas phase from Pd electrodes evolving primarily D_2.

We have $5.8.10^8$ disintegrations per minute per milliliter from gases which have been produced from low current density charging on such an electrode for two weeks.

The gases were passed over a recombination catalyst, and the tritium measured by standard method with particular reference to the avoidance of chemiluminescence.

The numbers are consistent with the number we earlier measured in the solution, and prove - rather, re-prove - the existence of nuclear reactions at the surface* of certain kinds of palladium electrodes in D_2O containing nickel ions.

Sincerely,

J. O'M. Bockris

JOMB/nm

rewritten by JB

* Note a change in viewpoint. The surface nature of the production is evidenced by the fact that when one makes the electrode an anode, dissolving, no further tritium is produced, i.e. none comes from the inside.

* Note a change in view point. The surface nature of the production is evidenced by the fact that when one makes the electrode an anode, - dissolving, - no further tritium is produced, i.e. none comes from inside.

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July 5, 1989

Dr. Jacob Bigeleisen
Department of Chemistry
State University of New York
Stonybrook, NY 11794-3400

Dear Dr. Bigeleisen:

Thank you for your letter of June 26.

We have now collected the gases coming off our cells, converted them to a tritium-containing liquid, and measured 10^8 counts per minute per milliliter.

I disagree with the thinking behind the last paragraph in your letter in which you refer to the difference in the rate of neutron production and tritium production as a "discrepancy".

There seems no doubt about the validity of our tritium measurements in the liquid directly produced after contact with a D_2 - evolving electrode for they have been confirmed by five individual organizations, and by two independent means here. It should be recalled that T is reported from Storms at Los Alamos at about the same order of magnitude; from Schoesser and Wellingford, University of Florida; from Grozzi in Rome and etc.

In respect to our measurements from the gases, we have not had time to have any confirming independent checks done.

There is no doubt about the neutron measurements. About the same result (a few times background) have been measured in five or six different laboratories.

These are the essence of the presently available facts, the most important of which is that the three types of observations occur sporacially and are thus not yet under control. However, there is absolutely no doubt that the positive observations repeat themselves better than one time in ten. It is up to the theorists to devise the model. Thus, some Pd electrodes, acting cathodically in D_2O, produce neutrons; and some produce tritium. We do no yet know if these events are coupled; nor whether they are associated with the sporadically observed heat production. But the phenomena are just 15 weeks old.

Again, thank you for your letter.

Sincerely,

J. O'M. Bockris

xc: Professor Norman Ramsey

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June 21, 1989

Dr. Jacob Bigeleisen
University of NY at Stony Brook
Stony Brook, NY 11794

Dear Dr. Bigeleisen:

I hope you have the tritium paper which I faxed you on the day you were here.

Of course, your view on our tritium measurements is more valuable than that of anyone else on the committee on cold fusion because you have the experience.

We want to regard these measurements only as facts, and we don't want to make any interpretation of them at this time. We need to know only that they are correct measurements.

In addition to the measurements which I showed you in the paper, I enclose a measurement from another electrode showing the change of tritium with time as the electrode continues its action.

It is, of course, important to consider the significance of these measurements, because errors such as one might think of - chemiluminiscence, etc. (very heavily guarded against in our measurements) - would hardly be likely to replicate the S-shaped curve which we have shown you in the first measurements, and which is to some extent replicated here.

It seems that the amount present can be fitted into a reasonable hypotheses concerning the origin of the tritium. One has to assume it is produced on the surface largely as bubbles, and produces some 5-10 watts/cc via the $D + D \rightarrow T + H$ reaction.

Then one can calculate the tritium which ought to be in the bubbles of gas as DT, and correspondingly by assuming equilibrium at high times the amount in solution.

Incidentally, I have experienced quite a few commissions of inquiry in my time, but I never found one which had its mind made up beforehand as much as this one.

I am sending you the present score as I know it on the confirmation of Fleischmann and Pons throughout the world, and I expect there's quite a lot that I don't know about.

I note the commission was out of touch even with the work going on in

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26 June 1989

Professor John O'M. Bockris,
Department of Chemistry,
Texas A&M University,
College Station, Texas 77843-3255

Dear Professor Bockris,

Let me thank you and your colleagues for your forthright presentation of your results on experiments relating to the possibility of cold fusion at our visit on 19 June 1989. I have a copy of your tritium manuscript. When I returned from our visits fof the week of 19 June, I had a message that you tried to call me on Wednesday, 21 June. I have tried several times to return your call to 409-845-4947 without success.

I thank you for your letter of 21 June sent via FAX. In that letter you mention a figure which gives an S shape curve for the rate of tritium production. There was such a curve for run A7 which was distributed with the copies of the overhead projections at the time of our visit. There was no figure attached to the FAX of 21 June. If there is an additional curve, please send it via regular mail.

The question as to whether tritium formed by cold fusion leaves the cell in the form of DT or whether most of it remains behind in the form of DTO is an interesting scientific question. We do know that all isotopic forms of H_2(g) exchange slowly with liquid water. A number of years ago Stevens at Chalk River achieved a major breakthrough and developed a non-wetting platinum catalyst which has exchange times for H_2(g) with liquid water of less than one second. I believe that there is a high probability that T formed at a Pd surface in a cell where the Pd is the cathode would exchange rapidly with the D_2O liquid. If, in fact, the exchange is slow, the the tritium production is 10^3 the rate found by analyzing the electrolyte. One should collect enough of the D_2(g) coming out of the cell, dry it and then measure its specific activity either in an ion chamber or a proportional counter (for a description of the latter see J. Bigeleisen and E.C. Kerr, J. Chem. Phys. 39, 763 (1989).

Our Committee is much interested in your tritium measurements and are troubled by the fact that your production rate for tritium atoms, as measured by the specific activity of the electrolyte, is some 10^{10} times the neutron counting rate measured by Kevin Wolf in similar type cells. If, in fact, most of the tritium leaves in the form of DT, then the discrepancy with the neutron data becomes 10^{13}.

Sincerely,
Jack Bigeleisen
Jacob Bigeleisen
Distinguished Professor Emeritus

xc: ERAB Panel

StonyBrook

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18 July 1989

Professor J. O'M. Bockris,
Department of Chemistry,
Texas A&M University,
College Station, Texas 77843-3255

Dear Professor Bockris,

I thank you for your two letters of 5 July with the information that you have found tritium in the offgas at a higher specific activity than in the liquid phase. I also acknowledge receipt today of your letter Of 20 June concerning tritium in the gas phase and pages 16-18 of a draft manuscript. You invite comment on these findings and your interpretation.

If the tritium in solution exists in the form of DT in equilibrium with that in the gas phase (the distribution being determined by the Henry's Law constant), then when a sample of D_2O is removed from the electrolytic cell and the D_2-DT atmosphere the dissolved gas will come out of solution and the liquid phase should lose its tritium activity, apart from the background. To check as to whether the tritium in the liquid phase exists as dissolved D_2, I suggest the following experiment. After the D_2O sample has been removed from the electrolytic cell and counted, the counting solution (with cocktail) should be flushed with tank hydrogen. This will remove all dissolved D_2 and DT and the specific activity should drop to that of the background initial D_2O.

In one experiment you have made the palladium electrode an anode after some time and find no additional tritium production. Whatever the status of the exchange rate of DT formed at the cathode with $D_2O(l)$, DT gas must exchange with the D_2 dissolved in the Pd. Therefore, I fail to understand that you do not drive tritium out of the elcetrode when you make it the anode. It is true that isotopic fractionation will lead to a lower tritium specific activity in the palladium than in the $D_2(g)$. This equilibrium has been studied as a function of temperature by Dr. M.W. Lee of the Savannah River Laboratory.

Sincerely,
Jacob Bigeleisen
Jacob Bigeleisen
Distinguished Professor

xc: ERAB Panel

Los Alamos
Los Alamos National Laboratory
Los Alamos, New Mexico 87545

7 AUG 89. O
-R.L. GARWIN-

DATE: July 25, 1989
IN REPLY REFER TO: N-1-89-698
MAIL STOP: E540
TELEPHONE: (505) 667-2182

Dr. Richard L. Garwin
IBM T. J. Watson Research CTR
P.O. Box 218
Yorktown Heights, NY 10598

Dear Dr. Garwin:

In response to your questions (7/12/89) concerning the neutron burst results from LA-UR-89-1974, the following information should help to clarify the issues.

1. The outside ^{3}He detector pods on System 3 have a detection efficiency(E) of ~1% compared with(E) ~34% for the inside detector.

2. The probability for detecting time-correlated (coincidence) neutrons varies as E^2, thus, the reals (coincidences) in the outer detector are ~1150 times less than the inner detector.

3. Applying this ratio to the case that you quote of 149 reals counts on 5/21/89, the number of reals in the outer tubes should be zero (149/1150) and we measured zero in the same 128 µs.

4. The reals rate in the outer pod is very low <0.4/h because of the low efficiency.

5. Part of your confusion concerning the data comes from not understanding the relationship between time-correlated counts (R) and number of neutrons (N) detected in less than 128 µs this relationship is

$$R = \frac{N(N-1)}{2},$$

thus when R=149, N=17 and the neutron source term is ~52 neutrons.

6. The Sandia counter is~9.3% efficient, thus the number of correlated counts that they will see is more than an order of magnitude less than our system, $(34/9.3)^2 = 13.4$. Also, we have four systems running in parallel resulting in a higher sample throughput.

7. In addition to these factors that the effect the throughput and success probability, our systems are second generation ^{3}He systems that are hardened against noise sources. The improvements in our systems include;

   a) The detector tubes are mounted into a sealed AL cavity that is designed to resist penetration from RF sources, line voltage spikes, moisture (via desiccant), and gamma-ray pile-up.

   b) An AMPEK solid state preamp/amp/disc. is mounted inside this Al cavity and directly above the ^{3}He tubes. This makes it possible to use 180 ns time constants in the AMPTEK to provide PSD against gamma-rays.

   c) The ^{3}He tubes contain a special $Ar+CH_4$ additive to shorten the neutron collection time.

8. A typical neutron burst from our experiment gives
   R=50 reals
   N~10 neutrons detected (128 μs)
   S~33 neutrons emitted.
   In the Sandia system, this same event would be
   S=33
   N= .093x33=3
   split 3 ways would give a split of 2,0,1 typically, and this event would be rejected by the redundancy criteria.

9. There are several approaches to distinguish between neutrons and noise. These include:

   a) Alternate between sample runs and dummy runs.
   b) Measure the 760 keV neutron spectrum using an MCA.
   c) Split the detector head into independent channels for comparison.
   d) Measure the time distribution of the burst and compare with the CH_2-detector die-away time.
   e) Use two or more separate systems in parallel for controls.
   f) Collect totals, reals, and accidentals and require that the rates are consistent with neutron bursts.
   g) Collect the bursts at a predictable time in the experiment.

The Sandia paper (J.E. Schirber et. al.) only used approach c) and they conclude that it is the best approach to isolate a neutron burst from a noise event. On the contrary, many noise sources will bypass this protection because the noise will enter all of the separate channels at the same time. These common mode noise sources include RF pickup, power line noise, cosmic ray bursts, and stray neutron sources.

In our experiments, we have used approaches a), d), e), f), and g). How much more credibility our results would have if we modified our system to include item c) is debatable. The strongest item on the anti-noise list is the last item g), and unfortunately this capability no longer exits with the Ti (662) alloy. However, for the electrolysis residues in the D_2 gas samples Ti-1 and Ti-6, we measured all of the bursts (12) between 2000-4000s into the LN warmup cycle. Before this set of experiments, we had observed no bursts over a four week period measuring Pons type D_2O cells in the same detector.

10. In response to your request for data for the control detectors. I have listed some of the results in Table I.

Table I

System 3 Burst Results with the Outside Pod Detectors

		System 3		System 3 Pod	
Sample	Date	T (cps)	R (counts/2000s)	(cps)	(counts/2000s)
DD-2	6/5/89	.253	83	.221	0
	6/6/89	.280	1087	.226	2
	6/6/89	.257	13	.225	0
	6/7/89	.256	47	.227	0
Ti-14	7/7/89	.253	619	.223	0
Ti-19	7/14/89	.250	412	.221	0

These results are consistent with the expected neutron rates in the System 3 central detector and the outside pod detector where the coincidence ratio between the inside and outside should be ~1000/1.

11. Recently (June 1989) we have added anti-noise feature d) to our System 3. The output digital signal is split to two separate coincidence electronic units. The one unit is set at 128 μs gate and the second unit is set at 16 μs gate. A ^{252}Cf source is used to establish the fraction of neutrons collected in each gate from an instantaneous neutron burst and (spontaneous fission). The measured ratio of reals in the long over the short gate is 3.178.

The sum for several weeks of background (cosmic ray) burst events is 128 μs/16 μs = 604/187 = 3.2 ± 0.3 and the sample neutron bursts events is 1614/637 = 2.53±0.4. These ratios both agree with the ^{252}Cf ratio and give additional evidence that the observed bursts are from neutrons.

12. Within the next couple of weeks, we will add the split (2-way) detector head feature that you suggested in your recent letter. This will be added to System 1 for mechanical convenience.

13. We plan to run the control experiment with H_2 gas in place of D_2 gas in the next few weeks. However, additional control samples are becoming evident in that we have run eight samples with D_2 gas and fully deuterided Ti or Pd and we have seen no neutron bursts from any of these pre-hydrided samples.

I hope this information is useful to you and the ERAB panel in evaluating our cold fusion results. If you have additional questions, please call (505) 667-2182 or 2187.

Sincerely,

Howard Menlove

H. O. Menlove

HOM:gec

ER-6

Cy: W. L. Woodard, DOE w/encl.
R. Jensen, ADR w/encl.
C. Orth, INC-11, w/encl.
R. Lemons, MEE-11 w/encl.
4RM-4 (2), MS A150
N-1 Files (2)

Santa Fe Meeting

Neutron Emission and the Tritium Content Associated with Deuterium Loaded Palladium and Titanium Metals*

Rough Draft
Sep 14-15
28D

K.L. Wolf, N.J.C. Packham, D. Lawson, J. Shoemaker,
F. Cheng and J.C. Wass

Cyclotron Institute and the Department of Chemistry
Texas A&M University, College Station, TX 77843

Abstract:

An experimental investigation has been conducted on samples of palladium and titanium metals which have been loaded with deuterium through the electrolysis of D_2O and by absorption of D_2 gas. In approximately 200 experiments on 25 cells, statistically significant evidence for neutron emission was obtained in three separate experiments from one palladium cathode. Observed rates are 3-4 times the background rate and correspond to a source strength of 50 neutrons/min. The pulse height response of the NE213 liquid scintillator-based detectors corresponds to that expected for 2.45 MeV neutrons. Tritium has been identified in nine Pd-Ni electrolytic cells, at levels corresponding 10^{12} to 10^{16} atoms. Activity buildup curves indicate that the apparent production occurs over a time period of several hours.

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072589.KLW

Introduction

The present paper reports some positive results for neutron emission and for tritium detection from the Fleischmann-Pons[1] type of electrolytic cells. The neutron emission corresponds to a source strength of up to 50 neutrons/min, similar to that reported by Jones,et al[2]. Most of the results are negative with respect to the detection of neutron emission in the 25 active electrolytic and gas phase cells investigated in the present study. The experimental program involved over 200 experiments with the variation of charging times and electrode potentials. Each cell configuration has a corresponding blank experiment with an identical cell which was unpowered or run with the electrode potentials reversed. Only one cell has shown clear indications of neutron emission and on three separate occasions. The results of tritium assays are more encouraging since nine cells have shown levels that are factors of 10^2 to 10^6 above background. Most of the experiments have been conducted on cells with palladium cathodes. The electrodes range from 0.5 mm to 6mm in diameter and were 4 cm in length. The electrolyte generally is 0.1 M LiOD in 99.9 % purity D_2O. Titanium metal rods have been used in 0.5mm and 3mm diameters. All experiments with titanium have proved to be negative, including high pressure D_2 gas cells at 1000psi and at liquid nitrogen temperatures.

The fusion reaction one assumes to be operating is the d + d + d--> $^4He^*$ reaction which produces 2.45 MeV neutrons and 1 MeV tritons with a nearly equal branches, as measured at deuteron bombarding energies where fusion is known to occur. The neutron experiments are sensitive to neutron energies of approximately 1-50 MeV to avoid an experiment of a highly specific nature, and to aid in measurement of cosmic ray background as described later. The tritium measurements use an integral, sampling technique with samples taken from the cell electrolytes and counted with liquid scintillator-based detectors. The possibility of tritium contamination from outside sources must be considered, as discussed later.

Experimental Method

The neutron experiments were performed with fast time-of-flight counters based on 3" X 5" NE-213 liquid scintillators coupled to 5" diameter RCA 8854 low noise phototubes. The timing feature cannot be used here, but the geometry of the design is well-suited to the low background requirements of the present work, through optimization of the pulse shape discrimination. The counter is located in a low gamma-ray background area but the pulse shape descrimination is needed to differentiate between neutrons and gamma rays, resulting in a background level of 0.5 c/min in the neutron energy range from 1-2.5 MeV. The lower energy level is determined by a constant fraction discriminator set at an electron energy of 0.35 MeV. The neutron response is lower than that of electrons due to saturation properties of the liquid scintillator. The efficiency is approximately 5% including the solid angle for 2.5 MeV neutrons. The contributions due to gamma-ray feedthrough into the neutron region of the pulse shape discrimination spectra are found to be less than 0.1 c/min which was established by testing with gamma-ray sources of various intensities. Conclusions of Gai,el al [3] about the inadequacy of pulse shape discrimination are found to be in error for the present situation and can be traced to an inappropriate design of the Yale neutron detectors.Sharp corners and long light guides result in light traps which decrease the rejection efficiency of the PSD method. The major contribution to the background level of the present experiment is found to be caused by the neutron component of cosmic ray showers. Most of the electrolytic cells were constructed for compatibility with the neutron measurements and allowed placement of the palladium electrode within 0.5" of the face of the neutron counter for high geometric efficiency and little degradation of the energy spectrum. The counter and cell were surrounded with 12" of parawax and protected on 4 sides with an active cosmic-ray shield of plastic scintillator. Charged particles originating from cosmic rays are rejected, and some neutron rejection is accomplished by the use of a gate veto of 5 microsecond duration to take advantage of the correlation of particles in cosmic ray bursts[4]. The system was protected from noise bursts with an antenna pickup system which was checked with a broad range frequency scan in the location of the electrolytic cell and in the adjacent laboratory where the electronics and the computer system were located. The detector was isolated from the cell with a 0.5 mm steel plate and an air gap, and the detector temperature was monitored. The K500 cyclotron was not in operation during the time of the experiments. Data were recorded with a Lecroy 2280 CAMAC system handled by an 80386-based small computer. The neutron efficiency and response were measured with three techniques, utilizing a Pu-Be source, a ^{252}Cf source, and a ^{252}Cf time-of-flight measurement. The latter technique was used to measure the pulse height response for a given energy neutron e.g. for 2 MeV neutrons. A solid state detector provided a 100ps reference which signals the occurrence of a spontaneous fission event for the neutron detector. A 1-meter flight path was sufficient to define the neutron energy to 10% or better. The neutron energy response calculation discussed later was tuned to reproduce the measured spectra, and the calculation was used mainly for the extrapolation to high geometry. This procedure allows a great deal of confidence in interpretation of the energy spectra, i.e. in knowing the response of the detector to 2.45 MeV neutrons.

Experimental Results

The analysis of the neutron data provides the counting rate as a function of time and an energy spectrum for specified time intervals. The system is rather insensitive to "bursts" unless the neutrons are within 200 nsec, corresponding to the gate time of the charge sensitive ADC system. Figure 1 shows the time dependence from the on-line analysis in a broad electron energy interval of 0.35-2.5 MeV. Despite the inclusion of extra background, the signal relative to the background rate is quite prominent, reaching 3.6 c/min compared to the background rate of 0.8 c/min. The data had been grouped into 15-20 min. intervals for preliminary analysis. A cell of identical construction had been used for blank runs prior to installation of the cell labeled JBA5. At an elapsed time of 170 min, the cell was installed and the cell current was increased from a low charging value to 140 ma. No further changes were made in the cell parameters. The cell was rotated a distance of 5"away from the face of the neutron counter at t=275 min. with a corresponding decrease in count rate, and a corresponding increase when returned back to the usual distance of 0.5" at t=300 min. The rate change is consistent with the efficiency loss due to the loss in solid angle, as expected if the neutrons were emitted from the cell and not from the surrounding material. The rate returned to the background level at t=340 min. with no change in cell parameters, signaling the end of neutron production.

The neutron energy spectrum lends more evidence to the contention that the emission of 2.45 MeV neutrons has been observed here. The pulse height data corresponding to the four points at t=230-310 min. in Fig.1 are integrated and plotted as the upper curve in Fig.2 which shows counts per 0.1 MeV energy interval as a function of energy, in electron energy units. The background curve is the sum of two time intervals each, before and after the neutron production corresponding to t= 140-175 min. and t=350-385 min., respectively. The background energy spectrum is much broader, as expected for secondary reactions in the surrounding material. In fact, the region above 0.8 MeV is used to tag cosmic-ray showers and provide evidence against interpretation of these data as cosmic-ray induced secondary reactions in the detector and in surrounding material. It can be seen in Fig.2 that the net number of counts in the region above 0.8 MeV is nearly zero, and indicates there was no unusual cosmic-ray activity. Of course this argument does not eliminate the possibility that cosmic rays may somehow initiate the d + d reaction in the cell, although there has been no model proposed that fits all of the data in the present paper. Comparisions with neutron response calculations gives good agreement with the present data, and mark the position of the expected broad edge in the spectrum at approximately 0.5-0.6 MeV. The results of a calculation[5] of the expected response is shown in Fig.3. A more modern calculation[6] of the predicted response is shown in Fig.4. The latter calculation was performed at ORNL without the benefit of the time-of-flight experimental data described earlier. The experimental data in Fig.5 and Fig.6 are in reasonable agreement with the calculations and suggest strongly that neutrons from the d + d reaction have been observed. The energy spectrum is quite different from that observed from fission spectrum neutrons, neutrons from natural alpha-induced reactions on light elements, and neutrons from cosmic ray shower-induced reactions in the surrounding material.

The response shown in Fig.2 and in Fig.5 is 9-10 standard deviations above the background level when an optimized energy integration interval is used (125 +/-13 counts). Figure 6 shows the energy spectrum for an earlier measurement on the same cell, taken with a different neutron detector (but of the same type) and different shielding arrangements (with 4" of iron surrounding the detector). The statistical significance is somewhat poorer due to a lower rate and a higher background level, caused mainly by cosmic ray reactions in the iron shield. The statistical significance corresponds to 4 standard deviations above the background (60 +/-15 counts). Again, the energy spectrum is indicative of 2.5 MeV neutrons. Of course the statistics relative to the average background is not the full criterion for determination of the significance of the neutron signal. One must compare the count rate relative to the fluctuations possible in the background, as determined by the cosmic-ray showers. Figure 7 shows a plot of the frequency of occurrence of the background count rate for the on-line data to be compared with values of the count-rate data in Fig.1. The long tail at the highest count rates in Fig.7 is of concern here, but one can see that there is a large margin of safety for the results in Fig.1.

Tritium results constitute the second topic to be covered in this paper. The measurements are quite different and much easier than neutron detection, with the major difficulties caused by the low energy of the beta emission and the possibility of external contamination. The activity levels found here eliminate the need for extensive discussions about backgrounds due to cosmic rays or gamma rays. The standard method of tritium detection at the present time is in situ water soluble liquid scintillation counting. A commercial LKB WALLAC 1219 counter was used for many measurements, and a rather good tritium counter was built at the Cyclotron Institute with a pair of low noise 2"-diameter phototubes situated on opposite sides of a 1cm X 1cm X 3cm rectangular cell, located in a light-tight box. A 50ns coincidence requirement minimized phototube noise contributions. The home-built system allowed careful beta end-point energy determinations to be made, which verified that the activity in question was tritium, and also allowed a careful search of electrolyte solutions for higher energy alpha emitters which proved to be negative. The activity levels as measured with the two systems at Texas A&M were within 0.5% and rather good agreement has been obtained by at least five outside laboratoties. The first outside laboratory to confirm the tritium result was General Motors Research[7], and researchers at LANL[8] have performed many variations on the sample preparation, the most definitive being vacuum distillation of samples to leave no doubt as to the purity. With the knowledge that tritium has appeared in the electrolytic cells with no chance that chemiluminescence has caused false signals, one must consider the possibility that the tritium was introduced externally.

Tritium has been observed in nine cells constructed in the Bockris laboratory, and many of the necessary blanks have been run, but not in sufficient numbers . Over 30 cells constructed in the Martin laboratory and those for the Srinivasan heat measurements have shown no nuclear effects, even though the cells have a somewhat similar construction with commom sources of many of the components. The major differences in construction of the Bockris cells include the use of nickel mesh for anodes, longer charging times, the use of small glass tubes for cell containment, the particular batch of palladium used, and the laboratory where cell preparation took place. From the data in hand, one must outline some quite special circumstances for explanations in terms of contamination. The background level of the heavy water is not a factor for the data presented in Table II as can be seen in Table I, which shows the results of approximately 10 determinations for each value quoted. Factors of 2-3 are at the limit for tritium bulidup from selective absorption which may explain the cell C-1. The total amount of D_2O added to the cells during the cell lifetime is only a few times the 15 ml cell volume. In the startup of the cold fusion experimental program, many of these cells were not assayed periodically. But those cells labeled C-C and C-G were followed and all solutions were assayed. The tritium appears rather suddenly over a time interval of a few hours as can be seen in Fig.8. The first point in Fig.8 was taken after the cell had charged at low current for several weeks, corresponding to the background level. At that time the current density was increased to 0.5 amps/cm^2 and the cell was assayed with 1 ml samples withdrawn every two hours. More D_2O was added as necessary from an assayed Aldrich sample. New hypodermic syringe packs were used for each transfer. A constant level was achieved after 12 hours which is probably indicative of the end of production. Strictly speaking, the constancy could reflect the equilibrium partial pressure of DT in the gas phase compared to the electrolyte concentration. The gas was free to escape through a tube immersed in mineral oil which prevents H_2O contamination from the atmosphere. This experiment was performed in the Bockris laboratory. In a second timed assay series, the results of cell C-G are shown in Fig.9 as a plot of activity per ml of electrolyte as a function of the date of assay. The cell had been constructed in the Bockris laboratory, charged and run at high current density and then transferred to a plastic cell and transported to the Cyclotron Institute while charging. An assay in the Bockris laboratory proved to be at the background level and a later measurement at the cyclotron was only slightly above background, which may reflect the use of different tritium counters for the measurements. Assays were performed daily on May 5,6 and 7 because the cell was situated in the neutron counter arrangement and the current density was increased in an attempt to correlate tritium production and neutron emission. During that period, access to the cell was quite limited since the neutron counter is located in a secure area on the experimental floor of the Cyclotron Institute, and the cell is futher protected by 12" of parawax which requires the removal of a shielding block which occurred only daily for assays and refilling of the cell. The rapid rise from the low count rate of May 1 and 5 to the elevated values of May 6 and 7 cannot be ascribed to outside contamination. Again all electrolyte and D_2O samples were assayed before addition to the cell. The decrease in the activity levels at long times is ascribed to an end of production and to losses due to displacement with D_2, since the cell was returned to charging conditions. Later the electrode was transferred to a new cell and electrolyte, and the current was increased to 0.5 amps/cm^2, but no further production was obtained. No gas phase recombination was performed. At the cyclotron the same syringe is used for a given cell in its lifetime to avoid the possibility that syringe packs are contaminated, since tritium is sometimes used by the manufacturers for checking the quality of needles. Also no tritium tracer work has ever been done in the cyclotron laboratories so the possibility of outside contamination is low.

A final important result comes from the Bockris laboratory in another cell that produced tritium after two months of charging.

Prepared at the time of most of the other cells shown in Table II, cell C-H (not shown in Table II) was charged and run at high current density, but with no indication of tritium above background. At that time a tube filled with platinized alumina beads[7] was fitted onto the cell for recombination of the deuterium and oxygen gases to water, and collection was made in a separate container. After two weeks of charging an assay of the recombination cell indicated 5 X 10^7 d/min/ml of tritium, and the electrolyte in the Pd-Ni cell contained 5 X 10^5 d/min/ml. Strictly by coincidence, both cell volumes were approximately 15 ml of D_2O. It had been suspected that large losses had occurred into the gas phasein the other cells, since measurements on recombination in the Fleischmann-Pons type cells have shown that less than 1 % of the gases recombine. The number of tritium atoms present here can be calculated to be approximately 10^{16}, and this was not the highest count in the electrolyte, compared to cells C-A and C-B in table II. It should be noted that the current density was not high (.05 amps/cm^2) during the collection time, although it had been run at 0.5 amps immediately before the installation of the catalyst.

Discussion of Results

The neutron results have shown three indications of 1-2 hour periods of neutron emission. One of these indications is over nine standard deviations in statistical significance and two indications are four standard deviations. All have occurred with the same palladium electrode. Over 20 cells of similar construction have given negative results. The energy spectra are consistent with those expected for 2.5 MeV neutrons, and the source is from the electrolytic cell. Some mechanism which involves the neutral component of the cosmic-ray spectrum cannot be ruled out, but no plausible mechanism has been suggested. The source rates of 20-50 neutrons/min are comparable to those measured by Jones, et al[2].

The tritium results show a much higher rate of reproducibility , since cells with eight 1-mm diameter Pd-wires and one 3-mm Pd-rod have shown significant levels. The calculated energy release rate based on the amount of tritium detected is significant, making the results even more important. If the production were through the d + d reaction with a Q-value of 4 MeV, the power level is approximately 10 watts/cc of palladium assuming a 5-10 hour generation period and using the activity levels of cell C-H.

The correlation of neutrons,tritium and heat has proved to be negative. Obvious heat generation (ignition) by a Sirinivassan cell was not accompanied by neutron emission, and these cells show no tritium production. Cell C-D has shown both neutron emission and tritium production, but it is not known if the occurrence was simultaneous. After reprocessing the electrode from cell C-D by melting the Pd, a positive neutron indication was observed but no tritium was produced at detectable levels. The measurement on cell C-G failed to give a neutron signal during the period of a positive tritium indication. If there is no delayed release mechanism for the tritium, one can state that the branching ratio for neutron production is too low by at least a factor of 10^5 compared to the observed tritium level in the electrolyte, for the d + d fusion reaction. Caution must be used in this conclusion because of the observation of tritium production in cell C-H well after a period of high current density. Neutron generation should occur due to the secondary reaction of t + d --> ^{4}He + n to produce 14 MeV neutrons even if the d + d reaction has a modified branching ratio or a different mechanism to avoid compound nucleus formation. The neutron detector system is quite efficient for 14 MeV neutrons and the energy spectra would allow easy identification, at a rate of approximately 10^6/min. The 3 MeV proton that accompanies the 1 MeV triton from d + d fusion would produce no neutrons since it is below the Coulomb barrier for Pd and is not of sufficient kinetic energy to breakup the deuteron (3.38 MeV). A search has not been made as yet for gamma rays produced by Coulomb excitation. The conclusion can be drawn, however, that if tritium is produced in the cells, an unknown mechanism creates the triton at a low kinetic energy to explain the lack of secondary reactions. Such mechanisms have been postulated, but there have been no detailed calculations performed.

An almost equally unlikely explanation occurs in terms of tritium contamination, but it is the one being pursued with tests and blanks in the cyclotron laboratories. The palladium cathodes originated from the same batch of material, and similarly for the nickel anodes. From the three samples which have been measured for buildup of tritium in the cells, it must be concluded that the tritium was introduced or was contained within a cell component at the time of cell preparation. Simarily, the observation of approximately 100 times the activity in the gas phase compared to the electrolyte indicates that the tritium was not introduced inadvertently into the electrolyte. The palladium is the most likely material for contamination because of its hydrogen storage capability, but all materials are being tested. Up to 10^{-4} of the hydrogen sites in the Pd must be preloaded with tritium to account for the yields found. This would be a considerable accident and would probably have required a trip to Savannah River for the cell C-H. Two H_2O, LiOH cells prepared from the same batches of the other materials (Pd,Ni,cells,rubber septa,etc.) have failed to produce any tritium. Samples of the Pd and Ni have been sent to LANL for direct analysis for preloaded tritium, but no results have been obtained as yet. Another type of blank was run by extracting the Pd wire from a cell that had charged for 25 days. The cathode was replaced with a 1 mm Pt wire. The cell was run at high current density and assays followed in the usual fashion as shown in Fig.10. No tritium above background was detected with this Pt-Ni cell. Figure 11 shows results for a cell prepared in the Bockris laboratory and run at the cyclotron. The negative result in itself constitutes a type of blank. The program of blanks and testing continues, but at present the strongest argument against tritium contamination is the magnitude of the yields detected at locations where little or no tritium is used, and the unlikeliness of contamination in the refining and manufacture of materials.

References

1. M. Fleischmann and S. Pons, J. Electroanal. Chem. 261,301(1989).
2. S.E. Jones, E.P. Palmer, J.B.Czirr, D.L. Decker, G.L. Jensen, J.M. Thorne, S.F. Taylor and J. Rafelski, Nature 338,737(1989).
3. M. Gai, these proceedings.
4. J.F. Ziegler, Nucl Instr. and Methods 191,419(1981).
5. R. Batchelor, W.B. Gilroy, J.B. Parker and J.H.Towle, Nucl. Instr. and Meth. 13, 70(1961).
6. J.K. Dickens, the program SCINFUL, ORNL-6462(1988).
7. D.A. Corrigan, Physical Chemistry, General Motors Research Laboratories
8. R. Sherman and collaborators, tritium facility, LANL.

TABLE I

TRITIUM BLANK SAMPLES, COUNTER BACKGROUND NOT SUBTRACTED

SAMPLE DESCRIPTION	COUNTS MIN^{-1}ML^{-1}
Millipore H_2O	23
Aldrich 99.9% D_2O	64
D_2O with KHP for pH adjustment	50
D_2O + 0.1mM NaCN with KHP	48
0.1 M LiOD	68
0.1 M LiOD with KHP	65
0.1 M LiOD + NaCN with KHP	66
ISOTEC D_2O	26

TABLE II

TRITIUM ACTIVITY FROM PALLADIUM - NICKEL CELLS

CELL	ELECTRODE TREATMENT[a]	ELECTROLYTE[b]	ACTIVITY (d min^{-1}ml^{-1})
C-A[d]	B	1	4.9 X 10^{6}
C-B	C	2	3.7 X 10^{6}
C-C	D	1	
	after charging at 0.05 amp/cm^2 for 4 weeks		64
	after 2 hours at 0.5amp/cm^2		5290
	after 6 hours at 0.5amp/cm^2		5.0 X 10^{5}
	after 12 hours at 0.5amp/cm^2		7.6 X 10^{5}
C-D[c]	B	2	1.2 X 10^{5}
C-E	A	1	3.8 X 10^{4}
C-F	B	1	6.3 X 10^{4}
C-G	A	2	
after charging at 0.05 amp for 4 weeks; 0.5amp,12 hours			120
after additional charging for 1 week,			250
after 0.1 amp for 24 hours, 0.3 amp for 1 hour			1.5 X 10^{4}
C-2 (3 mm)	B	1	6.3 X 10^{4}
C-3 (3 mm)	C	1	0
C-1 (6 mm)	A	1	69

a) electrode treatment: A, no treatment; B, vacuum anneal; C, acid etch; D, electroclean

b) solution type: 1, 0.1M LiOD; 2, 0.1 M LiOD + 0.1mM NaCN

c) cell JBA5 which has shown neutron activity, 50 n/min

d) verified by second ^{3}H counter at TAMU and by 5 other laboratories

All electrodes are 1mm diameter palladium except where noted.
A blank count rate of 65 c/min has been subtracted before calculation of activities.

Figure Captions

Fig.1. The observed count rate in counts per minute for a neutron counter as a function of time, divided into 15-20 minute intervals. The Pd-Ni cell was installed 0.5 " away from the face of the neutron counter at t=170 min marked as "A" and the cell was rotated away from the counter by a distance of 5" at the point marked "B".

Fig.2. The observed energy spectrum associated with the count rate data in Fig.1. The number of counts per 0.1 MeV in electron energy units is plotted as a function of electron energy as established with ^{137}Cs, ^{22}Na and ^{60}Co gamma-ray source Compton edges, and with triggered cosmic ray muons. Above 1 Mev the average of every 5 points is plotted.

Fig.3. The efficiency of the counters used here for 2.45 MeV neutrons as a function of energy in electron energy units, using the monte carlo calculation of ref.4. The crosses include the energy resolution of the detector and should compared to experimental data in Fig. 5,6.

Fig.4 The shape of the neutron energy spectrum expected for 2.45 MeV neutrons for the geometry of the present experiments. The calculation was performed by J. K. Dickens, ref.6. The abscissa is scaled in channels to correspond approximately to electron energy, 1 MeV = 100 channels.

Fig.5 The background subtracted data from Fig.2 (net) on the same scale as the efficiency calculations in Fig.3 and Fig.4.

Fig.6 The background subtracted data similar to Fig.5 for an earlier run on the same electrolytic cell.

Fig.7 The number of occurrences of a given count rate as a function of that count rate (frequency distribution) for evaluation of the data in Fig.1.

Fig.8 The count rate observed for cell C-C during liquid scintillation counting in a pulse height window appropriate for tritium beta decay, approximately 2-25 keV. Counter background (65 c/min) has not been subtracted. Samples of 1ml volume were withdrawn from the cell and mixed with 15 ml of liquid scintillator. All samples were counted again 24 hours later in a check for chemiluminescence. The zero of time on the abscissa was taken at the time of cell current increase from .05 to 0.5 amps/cm^2.

Fig.9 The count rate in (d/min/ml) in a tritium window(Fig. 8) for cell C-G in Table II as a function of date of assay. Notice that the abscissa is not linear. The current was increased on May 6 where indicated. Neutron measurements were made simultaneously.

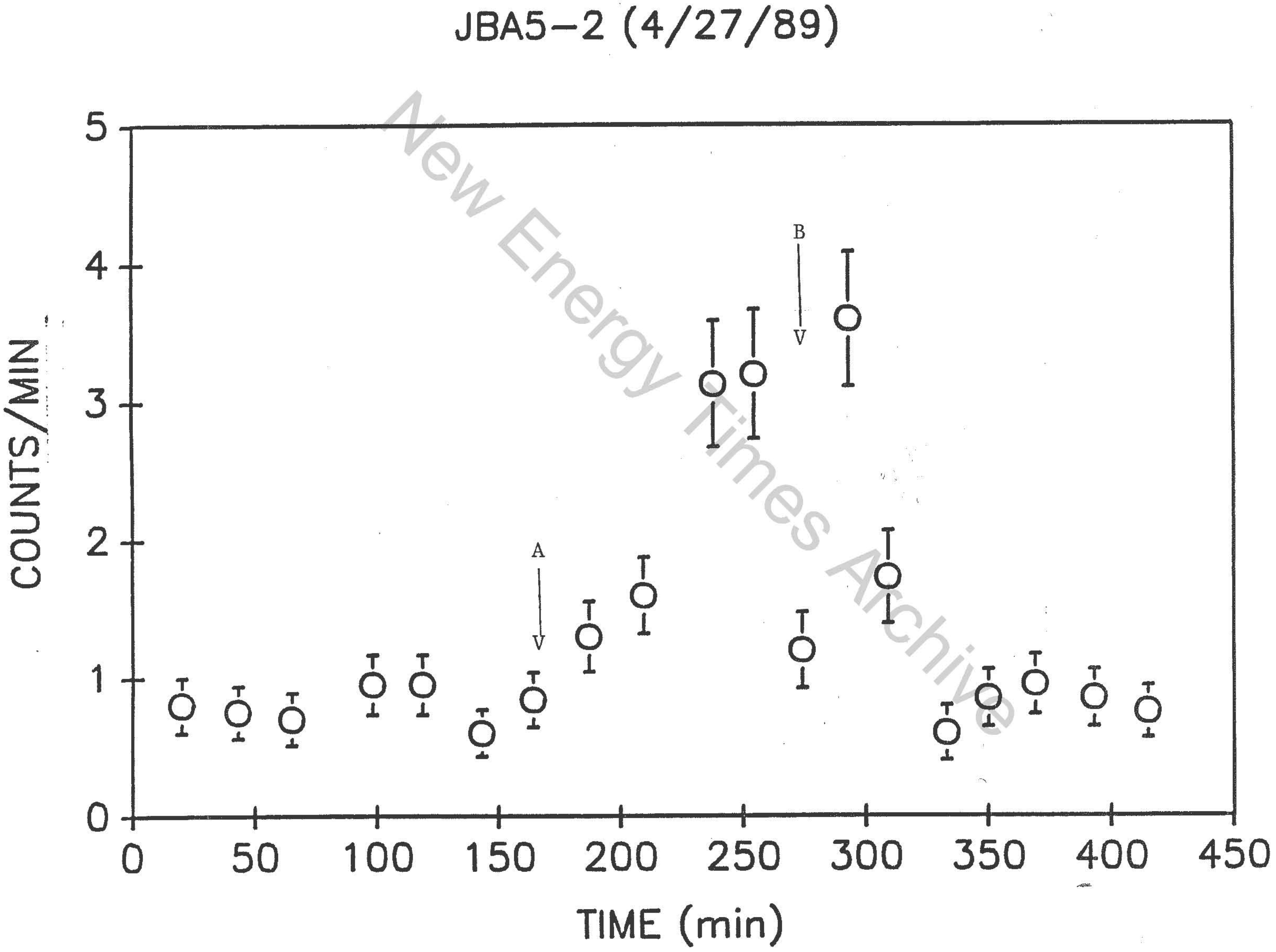
Fig.10 The count rate in a tritium window (Fig.8) as a function

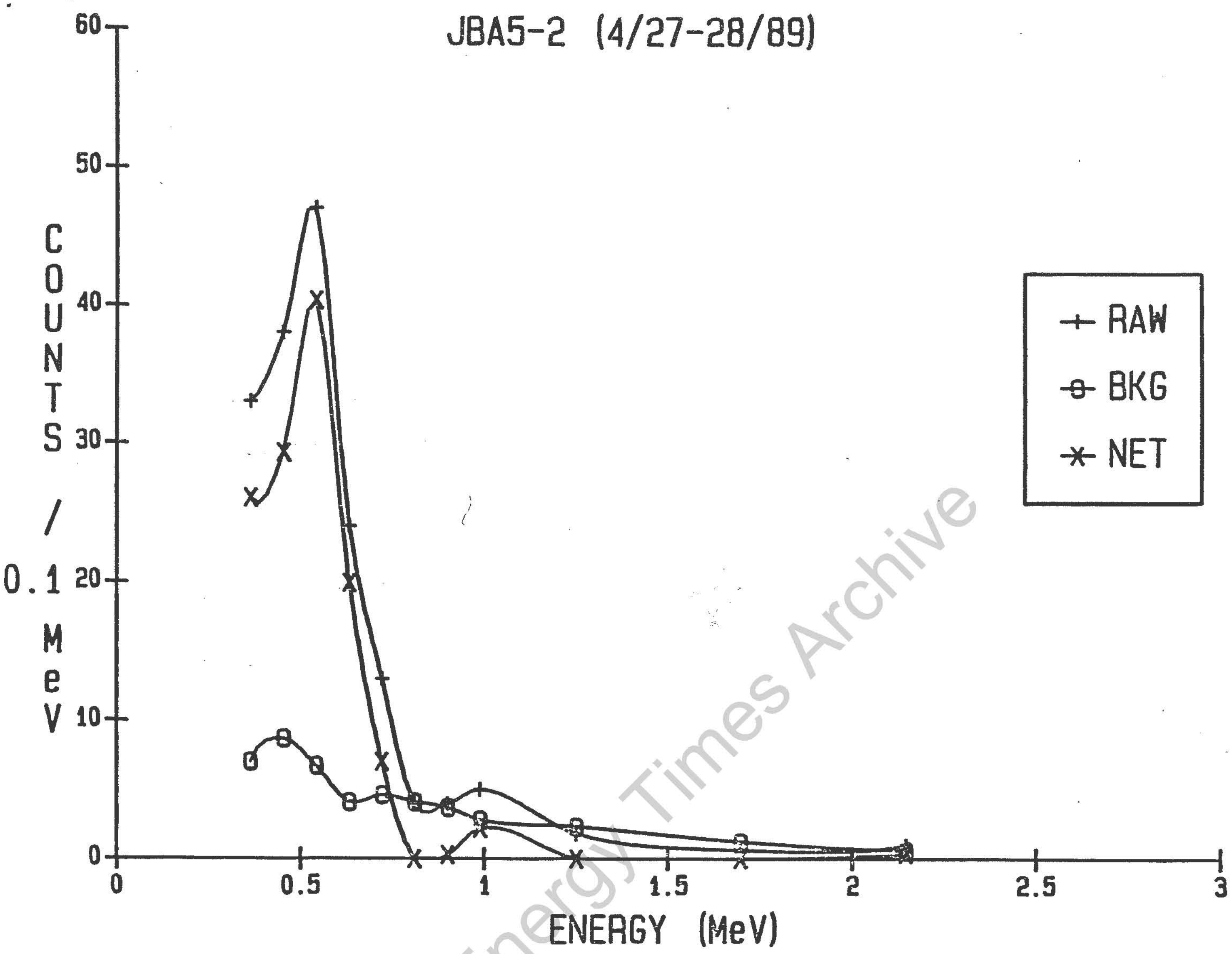
sample number, assayed approximately hourly for cell C-D in which the Pd-electrode was replaced with a 1mm Pt-electrode and the current density was increased to 0.5 amp/cm^2. ISOTEC D_2O was used (Table I).

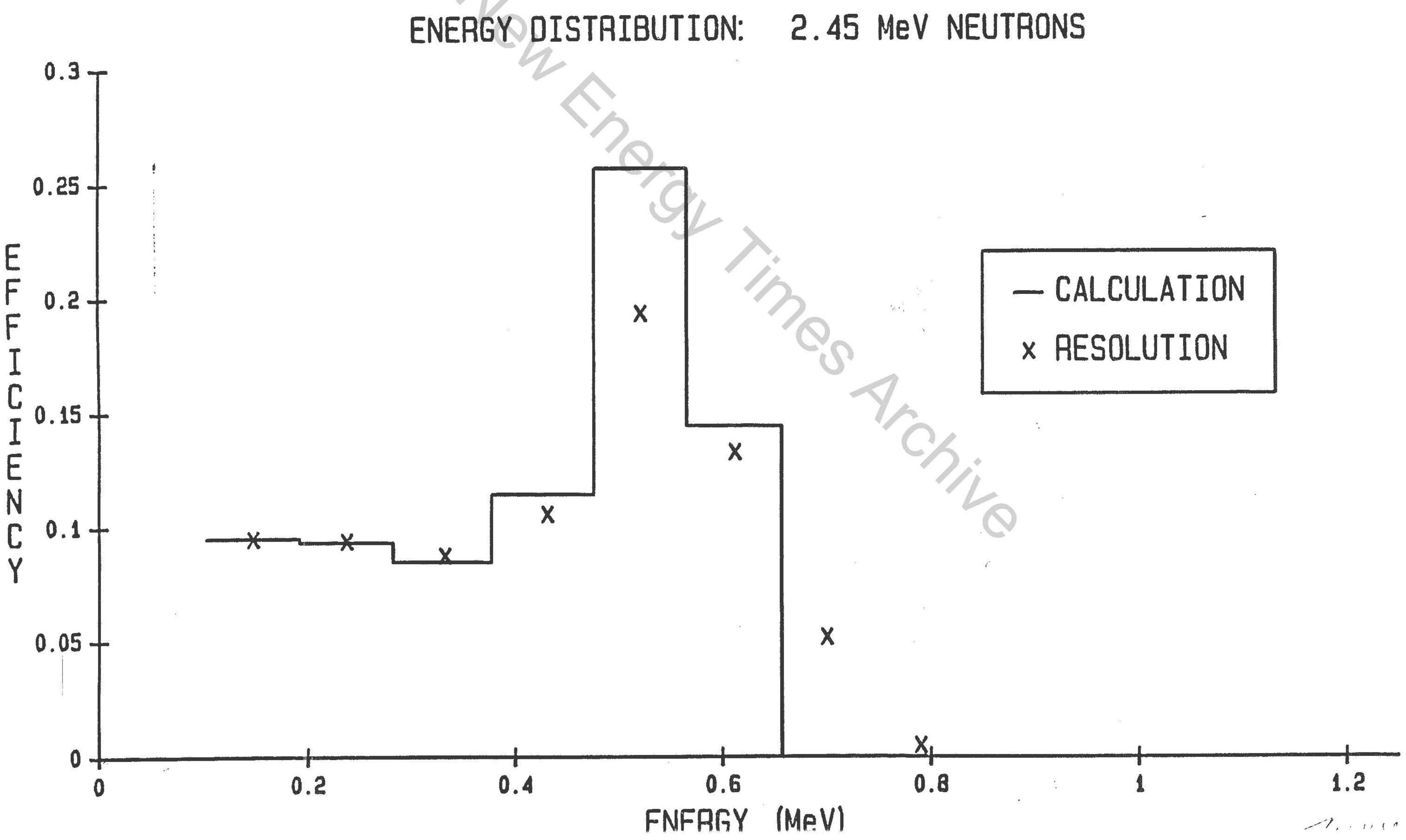
Fig. 11 The count rate as a function of sample number, taken approximately hourly for sample JB11. The cell contains a 3mm Pd rod similar to cell C-G in Table II. No counter background has been subtracted. ISOTEC D_2O was used (Table I).

ACKNOWLEDGEMENTS

The results presented here represent the efforts of approximately 45 scientists at Texas A&M University operating in five research groups, from nuclear science, chemistry and engineering. The efforts by J.O.M. Bockris and C. Martin were crucial for this project. The technical support by the Health-Physics staff was vital in counting and monitoring. The support by the Cyclotron Institute staff is gratefully acknowledged.


JBA5−2 (4/27/89)
COUNTS/MIN
0
1
2
3
4
5
0
50
100
150
200
250
300
350
400
450
TIME (min)
A
B


JBA5-2 (4/27-28/89)
COUNTS / 0.1 MeV
60
50
40
30
20
10
0
0
0.5
1
1.5
2
2.5
3
ENERGY (MeV)
RAW
BKG
NET


ENERGY DISTRIBUTION: 2.45 MeV NEUTRONS

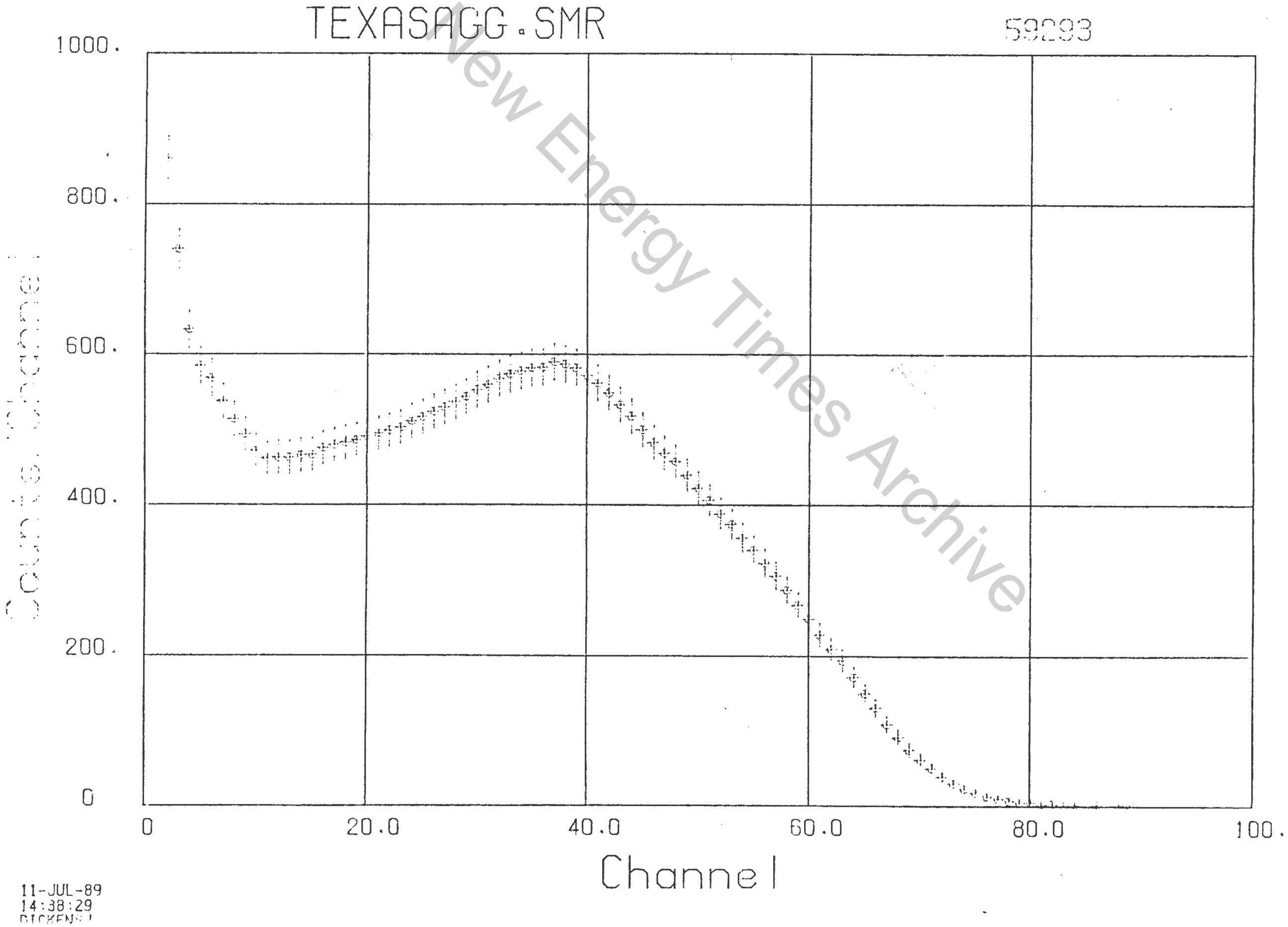
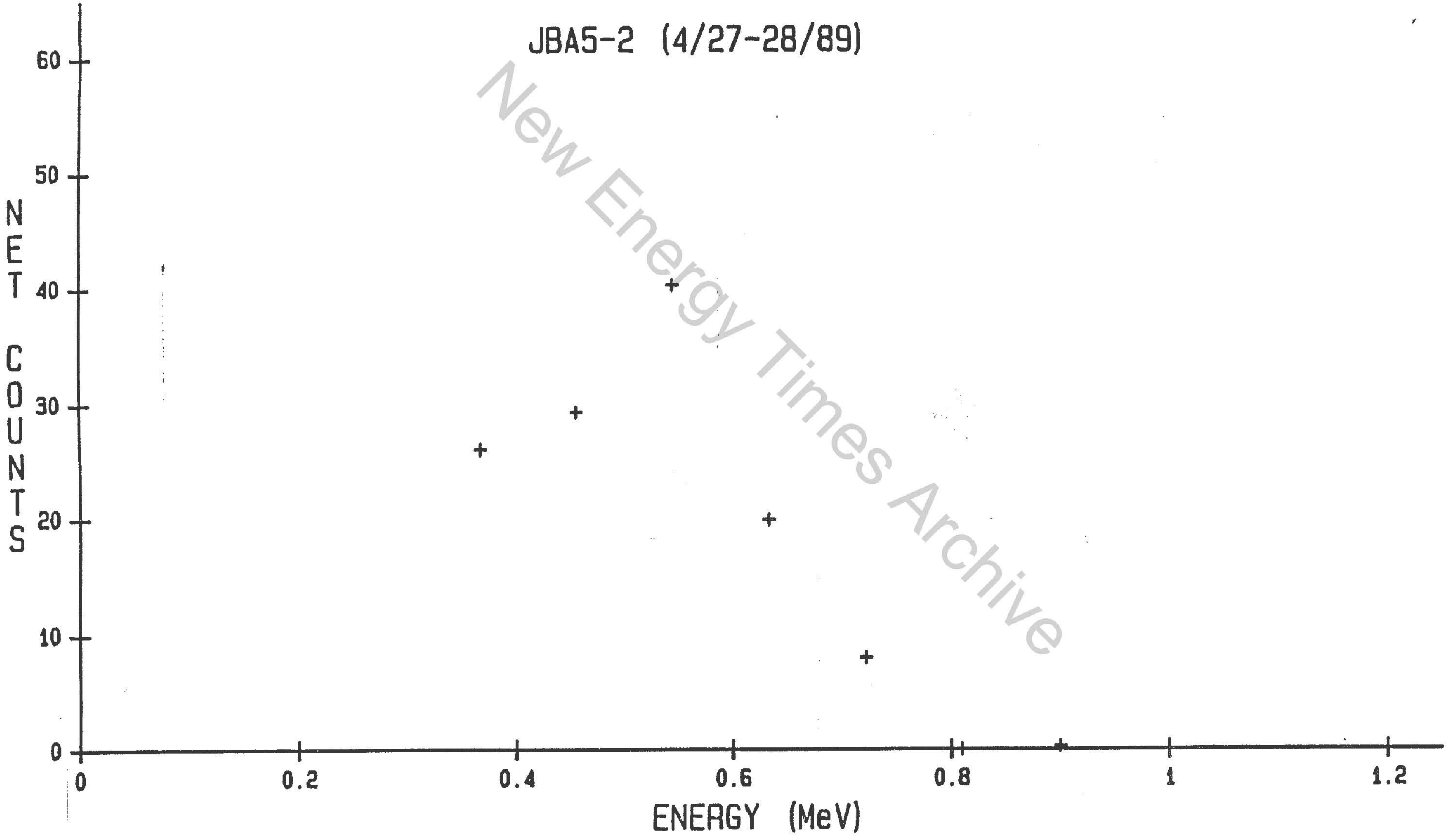
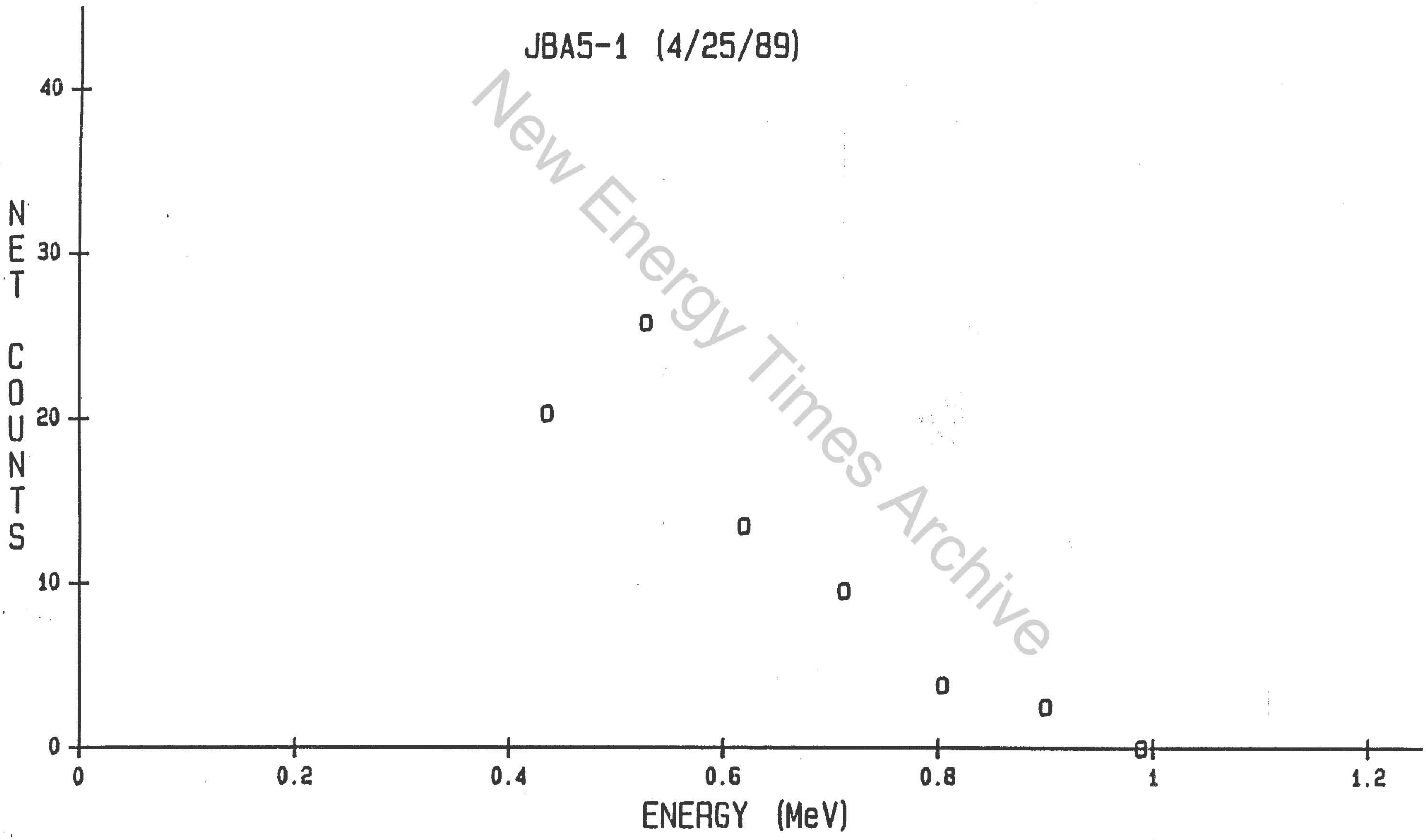

TEXASAGG.SMR
59293
1000.
800.
600.
400.
200.
0
Counts/Channel
0
20.0
40.0
60.0
80.0
100.
Channel
11-JUL-89
14:38:29
DICKENS

Figure 4


JBA5-2 (4/27-28/89)
NET COUNTS
0
10
20
30
40
50
60
ENERGY (MeV)
0
0.2
0.4
0.6
0.8
1
1.2


JBA5-1 (4/25/89)
NET COUNTS
40
30
20
10
0
0
0.2
0.4
0.6
0.8
1
1.2
ENERGY (MeV)

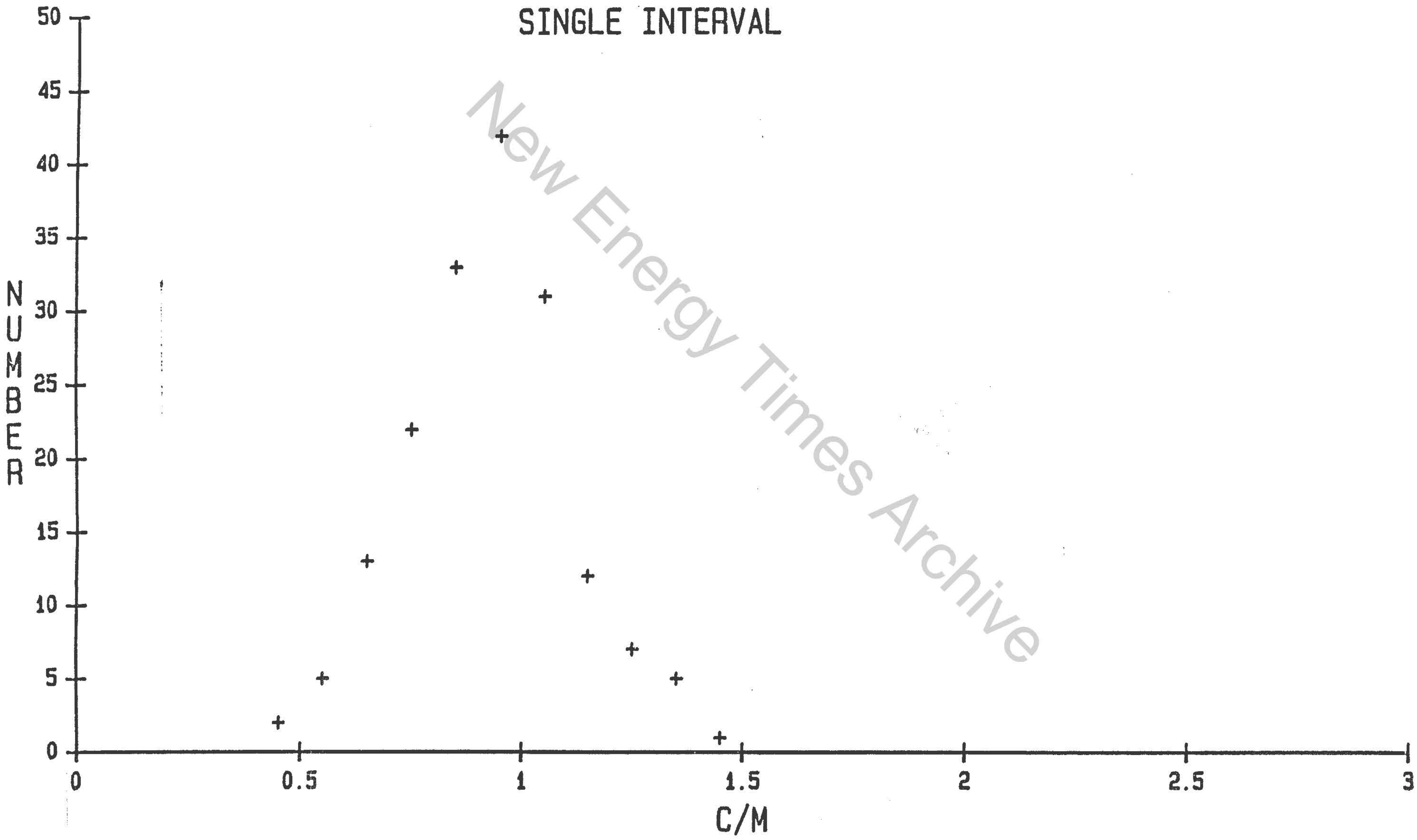
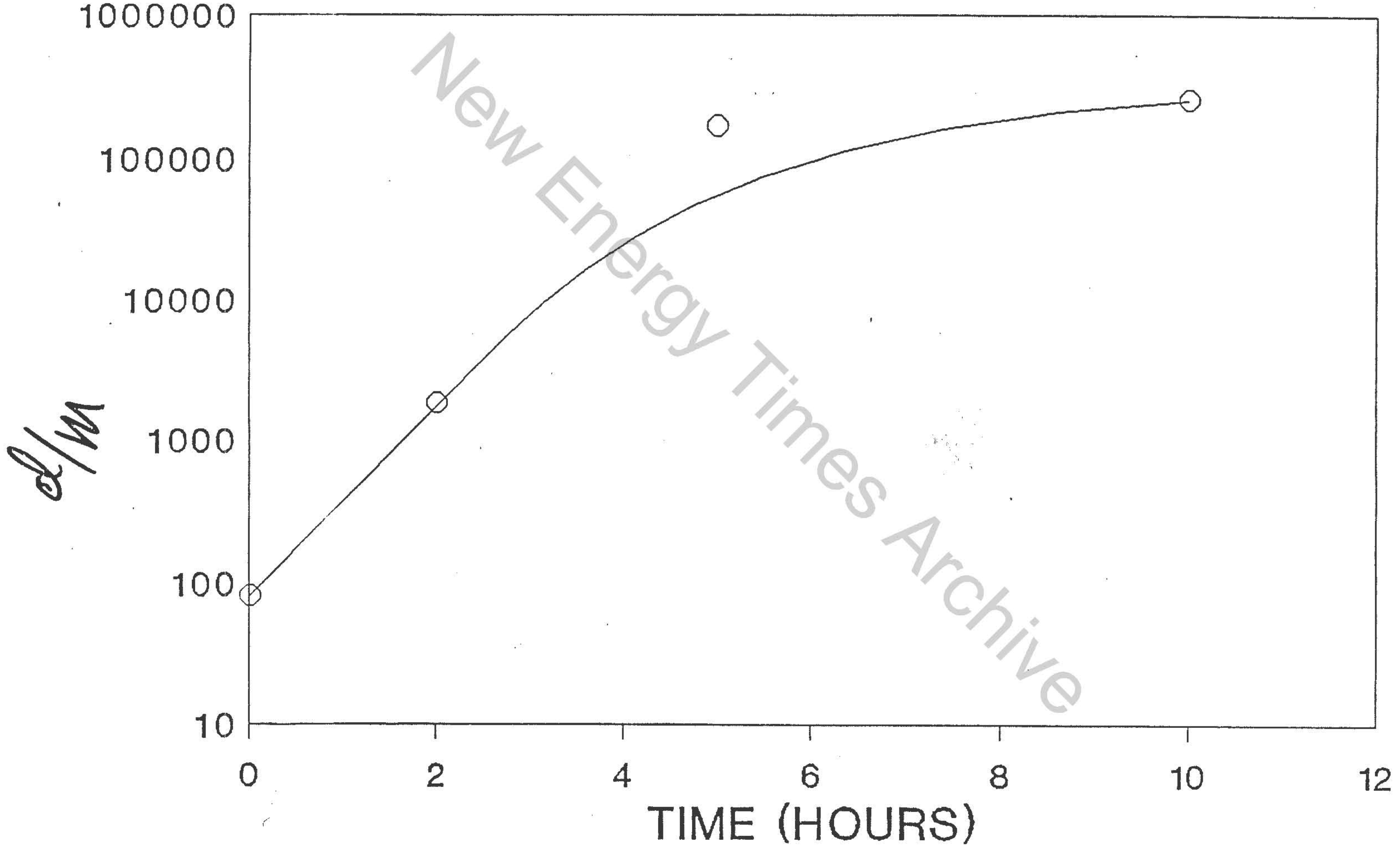

SINGLE INTERVAL
NUMBER
50
45
40
35
30
25
20
15
10
5
0
0
0.5
1
1.5
2
2.5
3
C/M

Fig 7

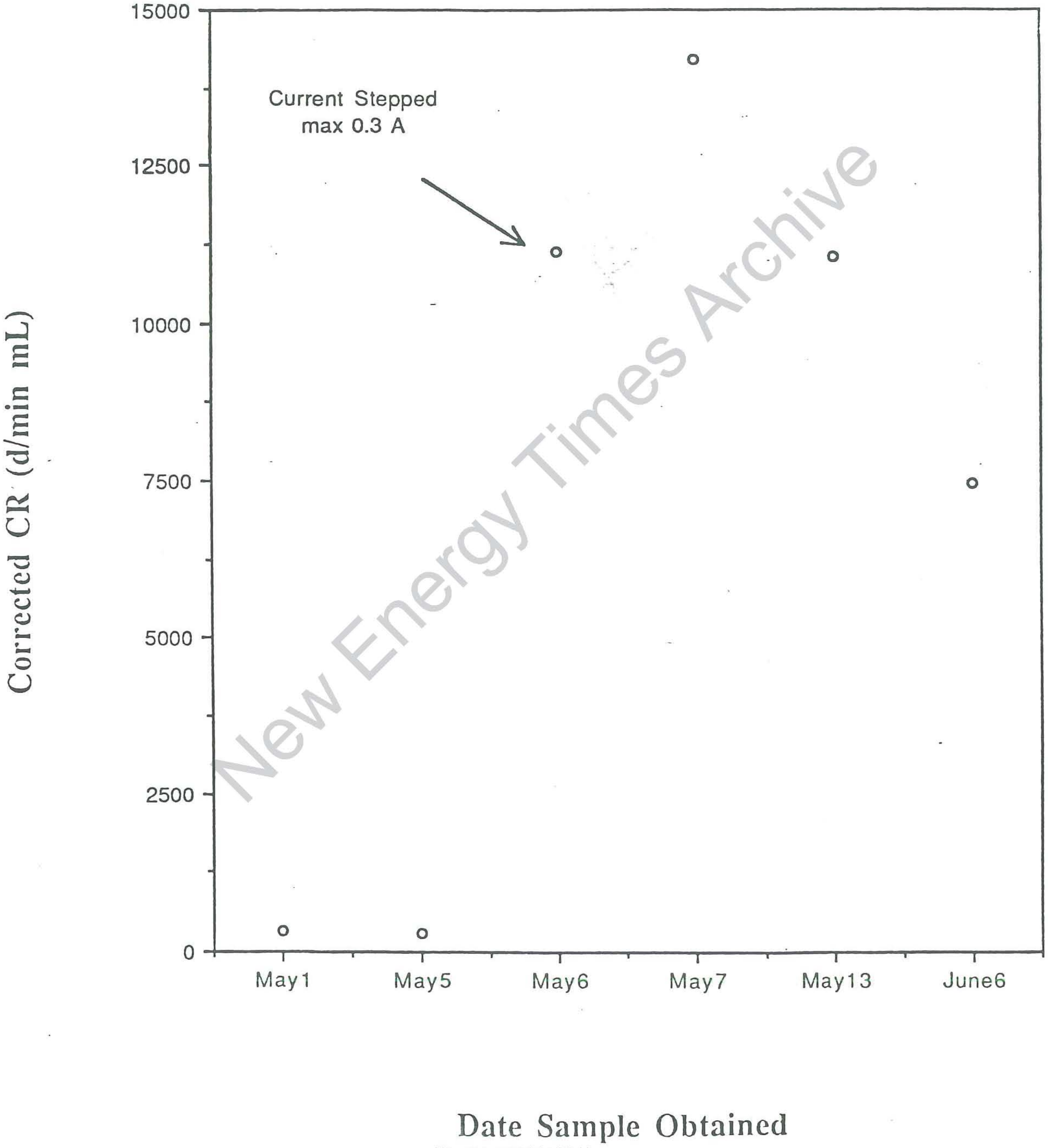
TRITIUM TIME PROFILE FROM CELL C

1 mm



7/14

Plot of Tritium Counts vs. Date Sampled for JBXR1



Tritium Counting - cpm vs. sample number

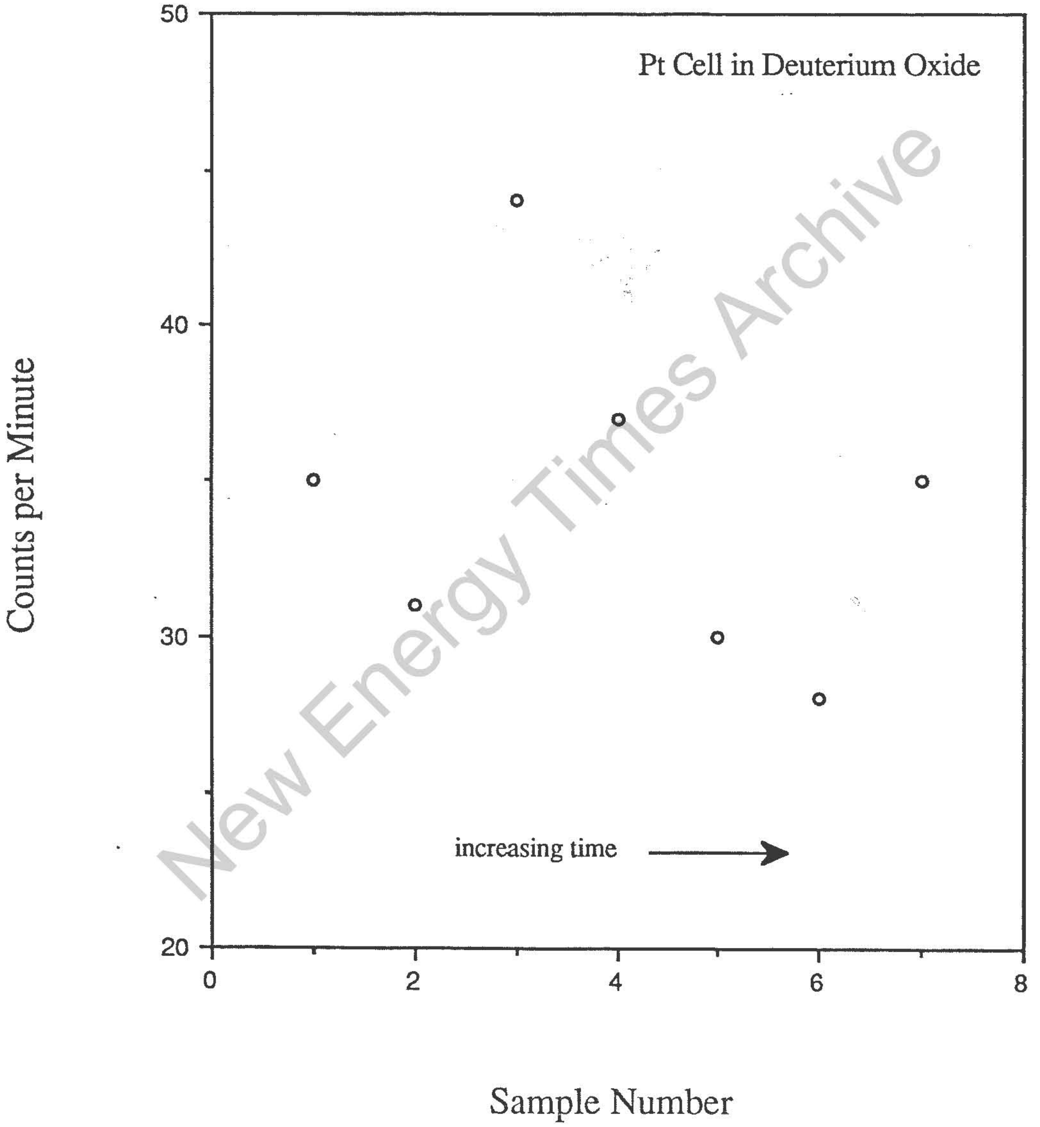


Fig 10

Tritium Counting- cpm vs. sample number

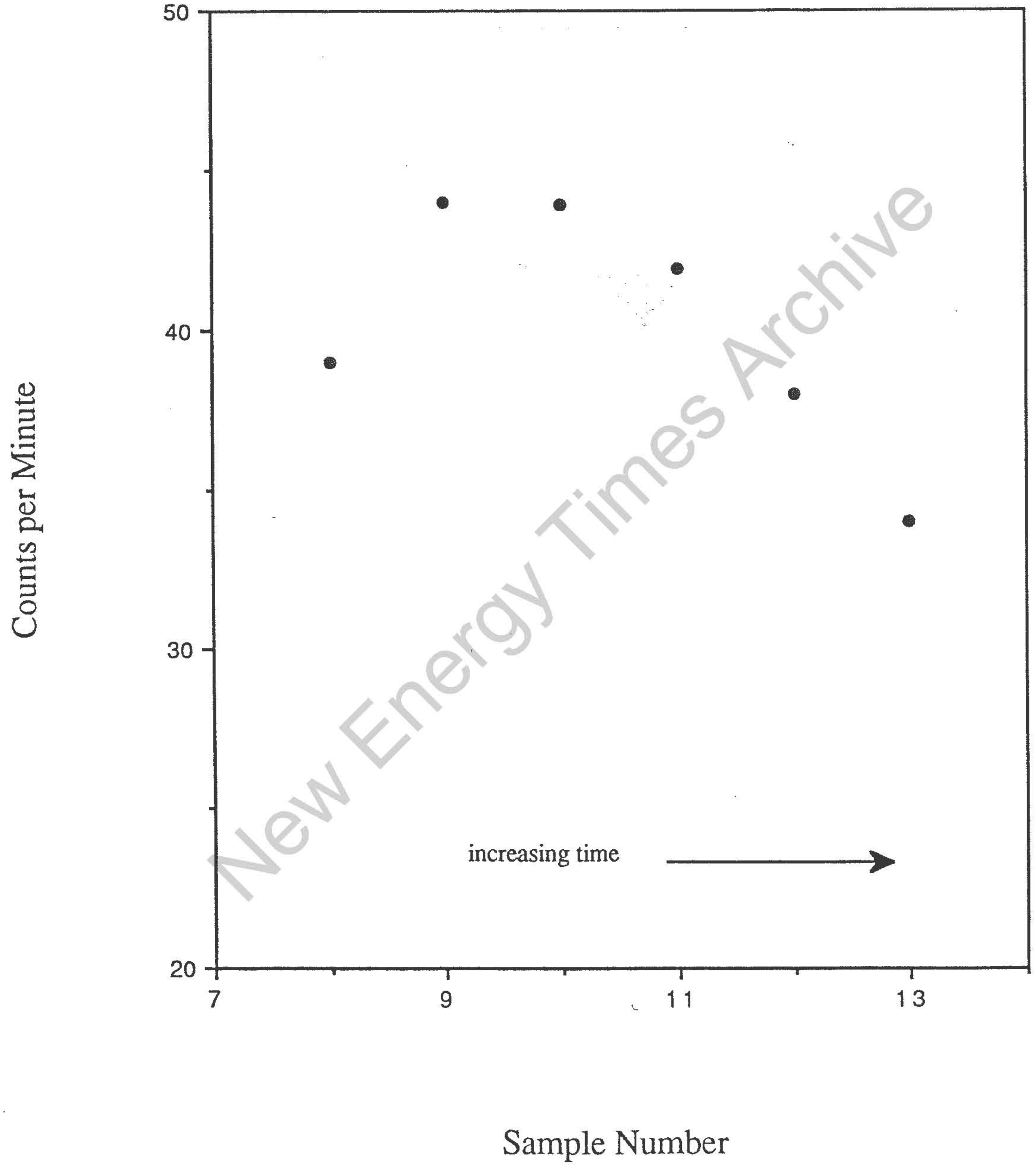


Fig 11

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

27 F

24 JUL 89 11. 44

-R.L. GARWIN-

July 21, 1989

To: Cold Fusion Panel

Enclosed are some press reports of last week's meeting and some assorted correspondence.

William Woodard

William Woodard

Enclosures

072189.WLW

Urges No Special Effort At DOE

WATKINS COLD FUSION PANEL UNCONVINCED BY PONS-FLEISCHMANN CLAIMS

For Cold Fusion File

INSIDE ENERGY 7/17/89

A DOE advisory panel last week said the sensational findings of cold fusion reported by University of Utah researchers earlier this year have not been substantiated and are not meaningful as an energy source. The group, formed to advise Energy Secretary James Watkins during the initial uproar created by the claims of Martin Fleischmann and Stanley Pons, recommended against establishment of any new cold fusion program at DOE.

"The panel finds that the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion. Indeed, evidence for the discovery of a new nuclear process termed cold fusion is not persuasive. Hence, no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time," the group said in an interim report.

John Huizenga, co-chairman of the cold fusion panel, which was formed under auspices of the Energy Research Advisory Board, declined to say the panel specifically ruled out the Pons and Fleischmann findings, but indicated he felt that the report stated so in different terms. The Utah researchers touted their findings for their potential as a new energy source, claiming their experimental apparatus was producing four times or more energy as was used to initiate the reaction.

The two researchers in March announced that they had discovered a new fusion process in which deuterium contained within heavy water is induced to fuse with other deuterium atoms within a palladium rod, releasing energy as heat. Since then, other laboratories, including many of the DOE national labs, have been frantically trying to reproduce those results, with almost uniformly negative results reported.

Huizenga, a nuclear chemistry professor at the University of Rochester, stressed the panel did not propose that cold fusion research be cut off. "There are unresolved scientific questions stemming from the cold fusion results," he said. "Some of these should be supported by further experimentation."

James Brophy, vice president for research at the University of Utah, said the university was undeterred by the panel's findings. "It doesn't surprise me that their findings are negative," he said, adding that he expects them to change by the time the group completes the final report in November.

Brophy said the university continues to stand behind the Pons and Fleischmann findings, noting that three groups at the institution have now confirmed the researchers' results.

Additionally, he said, "informal information" from both Oak Ridge and Los Alamos national laboratories supports the Utah findings. The two labs have detected heat, neutrons and large amounts of tritium, but their results have not yet been published, he asserted.

One of the troubling questions warranting further investigation is the reported findings of tritium in some of the experiments, the panel stated. The report noted that one experimental group found levels of tritium production in test cells that should have also resulted in comparable levels of neutrons, when, in fact, the observed neutron output was very small.

Indeed, measurement of neutrons, which the report referred to as "an established signature of the well studied (deuterium-deuterium) fusion reaction," never exceeded one neutron per second in any reported experimental finding. That rate, while possibly of some scientific interest, is 12 orders of magnitude, or one trillion times, below the number of neutrons required for one watt of fusion power.

None of the experiments to date have reported helium, another fusion product, above detectable levels, the panel stated. Helium should be detectable within the palladium cathodes used in the electrochemical cells if fusion is occurring.

The panel, which visited Pons' and Fleischmann's laboratories during their investigation, said they saw no operating cells that produced excess heat, a key claim of the Utah researchers. "So far, we have seen no experimental results that are sufficiently free of ambiguities and calibration problems to make us confident that the steady production of excess heat has been observed," the report said. However, some reports of brief temperature bursts are not presently understood and will require further evaluation.

The panel recommended that cold fusion research efforts in the area of heat production focus primarily on confirming or disproving the excess heat question. But it turned thumbs down on "significant expenditures" for establishing cold fusion research centers, such as has been proposed by Utah and some lawmakers, or for supporting new efforts to find cold fusion.

The panel further recommended that measurements be made for both fusion products and excess heat in the same test cell. Most, if not all, experiments to date have been set up to measure either heat or fusion products, but not both.

The panel also drew a distinction between the Pons and Fleischmann results and those of rival researcher Steven Jones of Brigham Young University. It said continued "modest" support for the BYU-type experiments, which have detected neutrons at very low levels, is justified, "provided the proposals for such research are evaluated in comparison with other DOE research proposals." — *David Kramer*

TEXAS A&M UNIVERSITY
DEPARTMENT OF CHEMISTRY
COLLEGE STATION, TEXAS 77843-3255

1876

(409) 845-2011
FAX (409) 845-4719

July 5, 1989

Dr. Phillip M. Stone
Director
Office of Science & Technology
Department of Energy
Washington, D.C. 20585

Dear Dr. Stone:

Thank you for your letter of June 26.

1) Composition of the committee

The experiments involved in cold fusion are electrochemical. Hence, to have 3 out of 22 persons representing electrochemistry is irrational. Eleven persons are physicists, - and the attitude of the Physicists with whom I have spoken, with the noted exception of that of Professor Ramsey, is that of emotional opposition to the recognition of the discoveries under investigation.

The three electrochemists would not be regarded by many physical electrochemists as representing that discipline. They have never attended the Gordon Conference in Physical Electrochemistry, and I think it unlikely that any one of them would be invited to speak at that conference (which I chair for 1990).

2) It would surely be agreed by all as unreasonable to include in the committee persons (Wrighton, Kunin, Bard) who have already expressed their negative decision publicly.

3) I now have a number of reports on interactions between member of the committee and the scientists carrying out successful experiments.

"I have never seen a set of men before with their minds so made up before they started."

"I have never been treated like this since I was a junior lieutenant in the Army."

"It was as though they were investigating a crime. I was afraid to say anything positive."

The value of a Report is proportional to its credibility. Composition of the Committee detracts from this: it should be modified before further damage is done to the attainment of an objective evaluation of the present extremely early phase.

Sincerely,

Dr. J. O'M. Bockris

RADIO TV REPORTS, INC.

4701 WILLARD AVENUE, CHEVY CHASE, MARYLAND 20815 (301) 656-4068

FOR DEPARTMENT OF ENERGY

PROGRAM The Today Show

STATION WRC-TV
NBC Network

DATE July 12, 1989 8:00 A.M.

CITY Washington, D.C.

SUBJECT Panel Discounts Cold Fusion Claims

JOHN PALMER: A panel of government-appointed scientists is reported ready to make public a report on the controversial scientific process called cold fusion.

Science correspondent Robert Bazell has been covering the investigation. He reports on the findings.

ROBERT BAZELL: Twenty-two prominent scientists assembled by the Department of Energy made up the first official group to examine the phenomenon called cold fusion. Members visited the laboratory in Utah where Dr. Stanley Pons and Dr. Martin Fleischmann claim to have created fusion in these jars, to have harnessed the reaction that fuels the sun.

Pons and Fleischmann hinted they may have found a cheap new source of energy for the world. Not so, the panel concluded in its draft report. The report said there is no convincing evidence that what is called cold fusion will ever produce significant amounts of energy.

Although many people wanted to believe Pons and Fleischmann, and some other scientists said they had repeated parts of the experiment, many scientists have predicted that cold fusion was too simple to be real.

DR. ROBERT PARK [American Physical Society]: It's a story the American people love. They love the story about the manager of the New York Yankees who spots some hillbilly throwing rocks at squirrels, and then takes him to New York to win the World Series. But, of course, it never happened.

BAZELL: It now appears that cold fusion will be

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remembered as one of the memorable mistakes in the history of science.

Robert Bazell, NBC News, Washington.

On Cold Fusion, It Looks Like Utah Stands Alone

NYT 7/16/89

By WILLIAM J. BROAD

THE probable death of low-temperature or "cold" nuclear fusion as a source of useful energy came last week as a panel of experts joined private skeptics by saying the startling finding was too good to be true.

The Energy Department panel, after searching the nation's laboratories for signs of cold fusion and deliberating nearly three months, concluded that the prospects of producing energy with cold fusion were so remote that no new laboratories to study the disputed phenomenon should be built by the Government or private groups. Further, it said no smaller research efforts of any kind should be initiated.

"Evidence for the discovery of a new nuclear process termed cold fusion is not persuasive," said the panel, adding that "experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion."

Nuclear fusion is the force that powers the sun, the stars and hydrogen bombs, fusing atoms together rather than breaking them apart as is done in nuclear reactors. It frees vast amounts of energy but usually requires temperatures of millions of degrees.

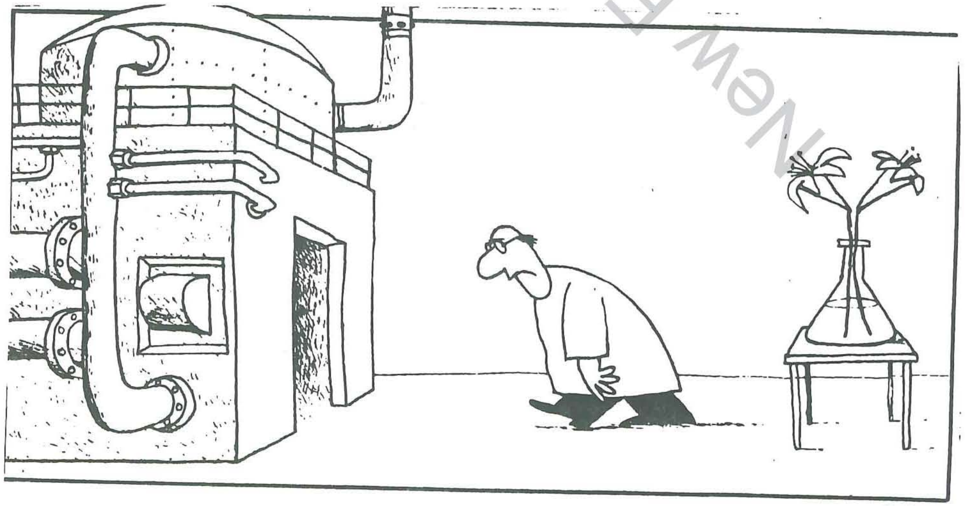
On March 23, two chemists, B. Stanley Pons, chairman of the University of Utah chemistry department, and Martin Fleischmann of Southampton University in England, startled the scientific world by announcing they had achieved fusion in a simple tabletop apparatus by passing an electric current through heavy water and palladium rods.

Undaunted by the Federal panel's skepticism, the University of Utah says it is pressing ahead with plans to set up a large cold-fusion research center. In recent weeks, it has been trying to persuade the Utah legislature to release $5 million it set aside for a laboratory to study cold fusion.

The funds are being withheld until the finding is "confirmed." A final vote on whether to release the money is set for Friday.

"We're still confident" cold fusion is real, said Dr. James Brophy, director of research at the University of Utah, adding that the Federal panel had rushed to judgment before all the evidence was in.

Indeed, the Federal panel's report was preliminary, although members said they expected the final report to be essentially the same, barring any surprises in the next few months.



Redinger

To: W.L. Woodard, DOE (Via FAX to 9-(202) 586-5119)

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

July 7, 1989
(Via FAX to 9-(415) 725-4034)

Professor Robert A. Huggins
Peterson Lab (Bldg 550), Room 550-I
Stanford University
Stanford, CA 94305

Dear Professor Huggins:

Thanks very much for providing us with an overview and discussion of your experiments, and for introducing us to your colleagues.

Two points arose in the discussion on which I believe it would be valuable to have specific information.

First, I understand that you repeatedly and at an early stage in the analysis determine K by adding 2, 4, or 6 watts of heater power to the electrolytic input, and determining the additional temperature rise. I would like to see for each cell these values of K plotted as a function of mean temperature. This would give us an indication of how much K varies with temperature and from one measurement to another in the same cell and at similar temperatures. So I would like to see the actual K points on a plot of temperature rise versus mean temperature for the measurement of that particular K.

Second, I would like to understand further the relevance of the differing conductivity of LiOH in H2O, in comparison with LiOD in D2O. Alan Bard indicated that there was about a factor 2 difference in conductivity, and I would like to see current data (and electrolytic power data) from one of your cells with light water or heavy water. Failing that information, I would like to see comparable numbers from two cells that are as similar as possible.

Third, you mentioned also some measurements of the distribution of electrical potential through an operating cell. I would like to see for a cell operating at reasonably high power this information as to potential drop in the anode layer, in the cathode layer, and through the bulk of the electrolyte.

It would be very helpful if you could send this to me by FAX to (619) 455-3943, so that I will have it in time to think about it before our meeting in Washington next Tuesday.

Thanks very much.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

cc:
W.L. Woodard, DOE (Via FAX to 9-(202) 586-3119)

RLG:jtml:188%RAH:070789.RAH

Dr. Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
FAX (619) 455 - 3943

July 7, 1989

Dear Dick:

I received your FAX just as we are on the way out the door to embark upon a trip for several days.

1. I am afraid that we do not have time at this moment to compile and plot the data you asked for on the temperature dependence of the cell calibration constant from our many experiments.

2. With regard to the electrolyte resistance issue that you raised in your second paragraph: you should recognize that the electrolyte resistance, along with the interfacial impedances, contributes to the sum of irreversible ohmic terms that are included in both the energy supplied and the energy released by the cell in this type of experiment.

The energy supplied = $E I t = (E_{rev} + E_{irrev.}) I t$.

The energy released = $(E_{rev} + E_{irrev.} + E_{excess} - E_{tn}) I t$

Therefore, since the irreversible terms are included in both the energy supplied and energy released, they will not affect the difference, which is a direct measure of the enthalpy change.

3. Our measurements on the distribution of the overall potential between the two interfaces and the electrolyte, which involve the use of a reference electrode, are very sparse to date, and were not under a wide range of conditions. We shall be undertaking more of these in the near future.

To give some indication of the ranges of total cell resistance in some of our experiments, from data on May 19 and 20, 1989, we find

For a hydrogen cell:

Power In	Cell Resistance
3.37 watts	16.2 ohms
8.512	11.865

For a deuterium cell:

Power In	Cell Resistance
3.435	21.683
8.569	15.948

In addition, there were some questions about values of power, calibration constants, output versus input power, spreads, etc. Perhaps the enclosed summary of data from our experiment on June 6, 1989, made on a deuterium cell that had been running continuously since May 18th, would be illustrative.

Please note that these calculations are based upon the total energy in, without subtracting the thermoneutral voltage, so that the Power Out to Power In ratios are of the most conservative type.

Also note that, since as mentioned above, the irreversible (Joule) terms occur in both the power in and power out values, one could increase the ratio of P_{out} to P_{in} by reducing those parameters. This is one direction to consider if one wishes to enhance the observed effects.

Bob Huggins

Stanford

R.A. HUGGINS

June 6, 1989

Stanford Experiment
D-Pd cell running since May 18, 1989

60689D5/Pd7

Power	ΔT/P Value	ΔT/P Ave	ΔT/P % Spread	Pout/Pin Value	Pout/Pin Ave	Pout/Pin % Spread
6.008	0.682	0.650	± 3.8	1.075	1.129	± 4.8
	0.644			1.138		
	0.625			1.173		
7.164	0.700	0.672	± 3.5	1.047	1.091	± 4.1
	0.668			1.098		
	0.649			1.129		
8.058	0.675	0.691	± 2.3	1.090	1.064	± 2.4
	0.691			1.063		
	0.708			1.038		
9.029	0.721	0.699	± 3.1	1.052	1.085	± 3.0
	0.677			1.119		
	0.700			1.083		
10.464	0.684	0.696	± 1.7	1.096	1.078	± 1.2
	0.704			1.065		
	0.699			1.072		

↑ Calibration Constant

↑

$$\frac{\text{Power Out}}{\text{Power In}} = \frac{K(\Delta T)}{EI}$$

This does not include the additional power out related to the chemical energy in the gaseous products.

The thermoneutral voltage was NOT subtracted, so this is excess power, even by the most conservative calculation.

27E

24 JUL 89 11. 44

-R.L. GARWIN-

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

To Cold Fusion Panel Members

Even though your meeting was one of the most boring events ever witnessed (according to the reporters and cameramen), and the disclaimer on the top of the report reads like we do not want to have anything to do with it, I wanted to thank each of you for the excellent report you produced. The report clearly shows what I saw at the meeting, namely, a group of highly talented people working very hard to convey the truth honestly and fully. You are performing a valuable public service and I thank you for it. Please keep up the good work.

My last point is a sad one. Our good friend and panel Technical Advisor, Dave Goodwin, lost his father on July 11. I am sure you all join with me in offering condolences to the Goodwin family.

Tom Finn

Tom Finn
Executive Director
Energy Research Advisory Board

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
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July 20, 1989

Dr. Claude A. Philis
Commissariat A L'Energie Atomique
Centre D'Etudes De Bruyeres-le-Chatel
Service Physique et Techniques
  Nucleaires
BP 12 - 91680 Bruyeres-le-Chatel
FRANCE

Dear Dr. Philis:

I was very pleased to have you visit me in my (temporary) home at La Jolla July 15. I hope that you had a good return to France.

In case you haven't received a copy of the note I sent Etienne Roth, I enclose one.

I write, however, primarily to send you the promised material on tritium, so that you will understand a bit better what is going on in the United States.

In addition, I am sending the Table of Contents of the book that resulted from our conference at the American Academy of Arts and Sciences December 9, 1988.

I must say that I do not share the enthusiasm and optimism for having tritium bear the burden of arms reductions, since I believe that we can and should reduce much more quickly than the decay rate of tritium. Nevertheless, I thought you might find this of interest.

Enclosed also is a report of the Belmont Workshop, dealing with Comprehensive Test Ban Treaty. I participated in the Belmont Workshop and find the report of very high quality.

Very best regards. I look forward to seeing you in Paris sometime soon.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

Encl:

+07/18/89 LRT RLG to E. Roth. (071889..ER)
07/12/89 LTR P. Leventhal to RLG including tritium materials. (071289..PL)
-12/00/88 Table of Contents of "The Tritium Factor - Tritium's Impact on Nuclear Arms Reductions" published by the Nuclear Control Institute and the American Academy of Arts and Sciences.
02/00/89 "Phasing Out Nuclear Weapons Tests," A Report to the President and Congress from the Belmont Conference on Nuclear Test Ban Policy, Natural Resources Defense Council. (020089NRDC)

RLG:jah:201%CAP:072089.CAP

27 G

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

July 19, 1989

TO THE COLD FUSION PANEL

Enclosed are the following materials from Dave Goodwin:

Attachment 1 - Background for the Interim Report

Attachment 2 - Material Provided by Dr. Huggins, Stanford University

Attachment 3 - Material provided by Dr. McKubre, SRI

Bill Woodard

24 JUL 89 11:44

-R.L. GARWIN-

071989.WLW

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United States Government **Department of Energy**

memorandum

DATE: JUL 18 1989

REPLY TO ATTN OF: ER-20.1

SUBJECT: Cold Fusion Summary

TO: W. Woodard, ER-6

Attachment #1 is provided as background for the interim report.

In attendance at the Stanford site visit, were Huizenga (Co-Chairman from Rochester), Callis (ACS President), Bard (Texas), Birnbaum (Illinois), Faulkner (Illinois)*, Fowler (Berkeley)*, Garwin (IBM), Happer (Princeton), Hoffman (Berkeley), Finn (ERAB) and myself (Technical Advisor).

*Did not attend SRI Site Visit.

Huggins provided attachment #2 and a tour of his lab.

Mc~~Krube~~ Kubre, et al provided attachment #3.

Art Nozik (SERI, FTS 327-1953) agreed to prepare, by Labor Day, the chemistry table Huizenga requested.

The Los Alamos cell which may have produced a 100 times increase in tritium, reportedly, may have been due to contamination.

On July 6, 1989, SRI/EPRI stated that Bockris reportedly now has a 10^6 increase in tritium.

In attendance at the July 11-12, 1989 Panel Meeting were Huizenga (Co-Chairman from Harvard), Ramsey (Co-Chairman from Harvard), Bard (Texas), Bigeleisen (Stony Brook), Birnbaum (Illinois), Callis (ACS President), Faulkner (Illinois), Fowler (Berkeley), Hoffman (Berkeley)*, Lipman (USGS), Miller (AT&T), Stein (Michigan Tech.) and Schiffer (Argonne)*.

*July 11, 1989 only

David Goodwin

David Goodwin
Special Assistant
Associate Director for
High Energy and Nuclear Physics

Attachment (3)

ATTACHMENT 1

COLD FUSION SUMMARY

Major U.S. Groups Reporting Heat

1. University of Utah: 2 groups (Pons, et al and Wadsworth, et al)

   Using different techniques.

   Site visit (please see attachment #1).

2. Texas A&M: 3 groups (Appleby et al, Bockris et al and Martin et al).

   Using different techniques.

   Bockris and 4th group (Wolf, et al) report tritium.

   Site visit (please see attachment # 2).

3. Stanford: Huggins, et al

   Using different technique.

   Site visit (please see attachment #3).

4. Stanford Research Institute: McKrube, et al

   Using different technique (D2 gas, no electrolysis)

   Site visit (see attachment #3).

5. Case Western: Landau, et al reportedly have 6 watts per cc excess heat and an up to 4 sigma neutron signal. Bockris stated they also have tritium.

Major U.S. Groups Reporting Neutrons

Brigham Young University: Site Visit (please see attachment #4)

Jones, et al: From both single neutrons and bursts of neutrons, both cells and gas, all confirmed by Los Alamos (Menlove, et al).

Other Major U.S. Groups

1. Brigham Young University:

   Bennion, et al: To measure neutrons, tritium and helium with a different technique.

2. IBM: Garwin stated they will try to reproduce the Italian experiment with gas which generated neutrons upon heating, instead of cooling.

Future R/D

Please see attachment #5

I. Experimental Configurations:

- o Nearly all groups to use recombiners.

II. Diagnostics:

- o Nearly all groups to analyze for tritium and helium.

III. Natural Processes:

- o Jones, et al with USGS to check deep undersea volcano.

Note: 12^+ groups plus referee testing implies up to $5 million/year for up to 3 years.

June 2, 1989 Site Visit to the University of Utah

At the Panel's request, the discussion focused on a working cell with a 1.25 cm by 0.4 cm Pd cathode and 64 milliamps per cm^2. Pons reported that, between 5.6 x 10^6 and 5.9 x 10^6 seconds (65 to 68 days) into the run, the excess power increased to up to 50 times input power and from an accumulated excess energy of 2 megajoules per cc to 8 megajoules per cc (this is about 1 to 2 kilowatt hours per cc, about 100 times that available from chemical reactions). Pons reported that between 2.6 x 10^6 and 3.2 x 10^6 seconds (30 to 37 days) after restarting the cell, the excess power initially went to 6.58 watts per cc, then to 12.6 watts per cc and then to boiling for several hours before being shut down (no measurements can be taken during boiling and the cell must be constantly manned for safety reasons). The accumulated excess energy, before boiling, was also several megajoules per cc. Pons made the following statements:

1. His heat monitoring is accurate to within 1% as confirmed by his H_2O control experiment and he will provide this data. He stated this is better than his previous control of a deuterium-loaded cathode in H_2O, which ruined a heat producing cell, due to the hydrogen.
2. He will send cathodes out for He-4 analysis.
3. His tritium comes in bursts, as he said Texas A&M reported, and his tritium is only about twice background because most is lost in the off-gas.
4. His measurements indicate he has a deutrium loading of greater than 1.0.
5. He will go to amps per cm^2. A flowthrough calorimeter and a calorimeter with a recombiner were shown in development for these experiments.
6. He does not provide specs to his Pd supplier and he uses the Pd, as is, from the supplier, (who reportedly uses a proprietary method to only cast, forge and draw the Pd.) He stated the one cc Pd cube, which vaporized, may have been a crystal, the Pd sheets he used next gave only marginal results and the first batch of Pd rods gave better results than the second batch he is now using. He (and the other Utah group) noted the importance of having little or no hydrogen in the rod.
7. He stated he does not use additives in the solution, he does not pre-electrolyse, he will try $LiSO_4$ and Li-6 and the other Utah group will try NaOH.
8. He stated the scale of many of the other experiments is too small to detect the temperature changes and that these changes are too small for isothermal calorimetry.

The independent, metallurgy group under Wadsworth, et al provided some of the May 1989, data on 6 heat bursts of 20 to 100% excess energy without subtracting the electrolysis energy, using 4mm by 10cm Pd rods, 100 to 120 milliamps per cm^2, after 18 to 19 day runs:

1. Power in: 9.7 watts, power out: 18 watts for 24 hours, 775,000 excess

joules, 58 times the energy available from burning all the deuterium in the rod.

2. Power in: 9.7 watts, power out: 58 watts for 91 minutes before an accidental shutdown.
3. Power in: 9 watts, power out: 58 watts for 40 minutes, 7 times the energy available from burning all the deutrium in the rod. After a small explosion, the power out went to 18 watts.
4. Power in: 6.3 watts, power out: 8 watts.

This group also stated:

1. They will do a control using a heat-producing, deuterium-loaded rod in H_2O.
2. They have sent out samples for tritium analysis.
3. They more worked rods produced only marginal heat.
4. Analysis of the cathode surface showed Pt, Silicates, Zn and Cu.
5. Analysis of the solution did not show H_2O_2 or "other chemicals."
6. They will use a doppler-shifted laser to crosscheck the thorough mixing indicated by their temperature measurements at different locations and at different input powers.
7. They will perform experiments with internal and external recombiners.

June 13, 1989 Site Visit to Brigham Young University (BYU)

Jones discussed the following results, stressing that single neutrons and neutrons bursts have both now been found in both electrolysis and in D_2 gas experiments.

Much of the discussion was on the recent reanalysis of the data which shows an up to 5 sigma, 2.5 MeV neutron signal based on about $1x10^6$ seconds of neutron producing electrolysis as compared with about $2x10^6$ seconds of background with H_2O, D_2O without current, no cells and empty cells. Jones stated that neutrons are produced after an hour, but, as he stated Los Alamos and Gran Sasso have also observed, the neutron production shuts down about 8 hours into the run. He reported that his collaboration with Gran Sasso has also produced 2.5 MeV neutrons.

He reported on bursts from electrolysis, of up to 53 neutrons in 128 microseconds from his collaboration with Los Alamos, who have one month of background data. He reported that Frascati is also observing bursts of neutrons from electrolysis.

During the Santa Fe Workshop, Jones noted that their effect tracked with the type of Li and they will try Li-6.

He next reported on D_2 gas experiments from his collaboration with Los Alamos producing 11 sigma single neutron signals and reproducible neutron bursts of about 150 neutrons in less than about 50 microseconds. He noted that Los Alamos cracking of loaded materials have not produced neutrons and that cracking does not explain single neutrons. He also noted a report of 7 MeV charged particles (from the Bureau of Mines).

During the June 22, 1989 ERAB meeting, Los Alamos (Menlove) stated that, using the very sophisticated and well established detectors in use for nuclear material safegaurds, they had 29 neutron bursts from gas and 6 from cells, consisting of 10^2 neutrons in << 100 microseconds, 10^2 background, not correlated with acoustical noise, up to 11 sigma single neutrons, that these results are reproducible and they will try D-T gas (100 times the neutron yield of D-D).

Jones discussed other collaborations, including an on-going experiment at LAMPF using a BGO crystal ball to search for 5.4 MeV gammas, plans to work with Gai (Yale) in August, possibly in September he will work with French who have a very sensitive neutron detection system, and arrangements being made with the USGS to search for tritium in the glass of deep undersea volcanos (not affected by bomb tests). He discussed a new BYU experiment that will use neutrons to trigger examinations of neutron and gamma bursts with one microsecond resolution.

He concluded by stressing that he has effects and potential applications, particularly if the neutron bursts can be enhanced, that are separate from heat and he noted that he does not use rods. He noted that our time did not permit a discussion of astrophysics. During the press conference, he stated the BYU patents are based on using his effect as a neutron source.

An independent group under Bennion is preparing a 100 to 200 AMP/cm^2, 1-2 KW experiment designed to run for extended periods, using a recombiner to contain the gases, with monitoring for neutrons, tritium, He-3 and He-4. Bennion stated that if it is a surface effect, the helium should be in the gas instead of in the electrode, and he also noted Jones has substantial Li, Zn, etc. on the electrode surface.

June 19, 1989, Site Visit to Texas A&M

Group #1

Appleby reported on the following experiments:

1. Three experiments with the following excess heat generation:
   (A). 3 runs: 16.3, 19.3 18.5 watts/cc
   (B). 4-7 watts/cc
   (C). 6-12 watts/cc
2. Two experiments where:
   (A). Excess heat generation was nearly eliminated by switching from LiOD to NaOH and
   (B) Excess heat generation returned by switching back to LiOD.
3. An experiment where excess heat generation was nearly eliminated by switching from an uncoated stainless steel cell to a teflon-coated Ni cell.
4. An experiment with LiOH, instead of LiOD, which generated no excess heat.
5. An experiment with Pt in LiOD, instead of Pd in LiOD, which generated no excess heat.
6. An experiment where:
   (A). Excess heat generation was nearly eliminated by switching from 6% Li-6 to Li-7 and
   (B). Excess heat was generated faster by switching to Li-6.

Analysis shows Li penetrates about 1 micron into the electrode. Surface analysis, at least some of which was done by Livermore, indicates Fe, Cr, Mg, Ca and Si. Bockris noted that Ni is also present.

Appleby also noted they have recently began measuring gas volumes in one experiment to confirm low levels of recombination.

Group #2

Packham, from the Bockris group, discussed 8 positive results for tritium, showing increases up to > 10^4 and that tritium analysis have been confirmed by sets of 5 samples which were sent to Argonne, Pacific Northwest, Los Alamos and General Motors.

Bockris discussed:

1. His 3 cells with excess heat: 4.5, 5.0 and 7 watts/cc,
2. That he is installing recombiners in 1/2 of his cells,
3. That his calculations from Henry's Law indicate about 100 times more tritium in the gas than in the electrolyte, which is approximately the level of tritium suggested by the excess heat, and
4. That Fe/Cr and Ni dendrite tips may produce the KeV energies required for fusion.

The Bockris, et al paper, which was provided during the June 22, 1989 ERAB meeting, stated that they observed heat for up to 33 hours.

Group #3

Martin reported on :

1. Two cells, one with up to 50% excess heat,
2. That measurements of gas flow indicated recombination does not exceed 1%, even for exposed electrodes
3. Collaboration with Vancouver to use their flow-thru calorimeter which has a recombiner, and
4. Surface analysis shows high levels of Li and increased levels of C, Si and O.

Group #4

Wolf provided 3 positive results on neutrons from the Texas A&M, including 2.5 MeV neutrons up to 4 1/2 times background. The highest neutron level corresponded to an over 10^3 tritium increase. He also presented surface analysis which indicated (probably Fe based) dendrites and cracks. He also discussed the possibly of D + Li-6 fusion.

During the June 22, 1989 ERAB meeting it was reported that:

1. Martin reported that the unworked electrode produced the 50% excess heat.

2. Bockris reported only Ni (not Pt) anodes produce tritium.

3. Wolf reported he had one tritium-producing cell.

4. Los Alamos distilled the Bockris sample and still found tritium.

On July 6, 1989, SIR/EPRI stated that Bockris reportedly now has 10^6 increase in tritium.

July 6, 1989 Site Visit to Stanford University

Huggins made the following points:

1. Need at least 3 volts of potential difference (He operates at 3 to 15 volts).

2. Need 10 to 1000 milliamps per cm^2.

3. Must remove, and keep removed H, O and H_2O. (He has observed decay of excess power with H_2O).

4. He has one cell with a recombiner.

5. He uses coin-shaped Pd cathodes suspended on Au wire and Pt wire anodes.

6. Surface deposits affects the results.

7. "All other exothermic processes did not contribute more than 7% of the total measured temperature rise."

8. "Heater calibration of cell constant 3 times at each electrohemical power level."

9. "Excess power depends upon...supplied power."

10. The "excess power...varies with time...implies that it happens inside solid, not on surface...increase with time...would (not) expect if some chemical reactant were being consumed...(no) time-dependence...(with) H."

11. He intermittingly produces about 1^+ watt of excess power per gram.

12. He will work with Los Alamos to monitor tritium

Privately, Huggins stated he will also try using materials less expensive than Pd and higher operating temperatures (which will also reduce the relative background).

During the Santa Fe Workshop, Huggins reported 22% excess heat, agreed to have Caltech test a heat producing cell, and have others test a heat producing cell for tritium, helium-3 and helium-4.

July 6, 1989 Site Visit to the Stanford Research Institute (SRI)

Using a closed system consisting of D_2 gas at 400 PSI, an LiOD electrolyte without electrolysis (< 1.5 volts), 10^2 milliamps/cm^2, a Pd crystal rod and a Pd coated Cu rod (as a reference cell), SRI obtained a D loading of 1.0 and about 1 watt of excess power for 7 hours, with little or no surface contamination. McKrube noted that the Pd coated Cu cell sometimes produced a lesser amount of excess heat, indicating a possible surface effect and that he will also use a flowthru calorimeter.

Future R/D

Basic Experimental (and theoretical) multi-disciplinary research should include:

1. Experimental configurations: e.g.
   - I. Calorimetry methods
   - II. Electrode preparation methods (and analysis); including materials, form, size and additives
   - III. Electrolytic solutions (including ditives and analysis)
   - IV. Current densities
   - V. Charging times
   - VI. Deuterium loading (and analysis)
   - VII. Removing hydrogen and water
2. Diagnostics: e.g.
   - I. Neutrons and gammas (and X-rays), including detection of bursts
   - II. Tritium, helium-3 and helium-4, including cathode, solution and off-gas
3. Natural Processes: e.g. geophysics and astrophysics

ATTACHMENT 3

MEETING TO REVIEW STATUS OF EPRI-SPONSORED ELECTROCHEMICAL RESEARCH USING A PRESSURIZED CELL WITH HEAVY WATER ELECTROLYTE

JULY 6, 1989
EPRI, BUILDING 8, ROOM 248

AGENDA

1:30 P.M.	INTRODUCTIONS AND OPENING REMARKS	T. PASSELL EPRI
1:40 P.M.	SRI PERSPECTIVES	D. MacDONALD SRI INT'L
1:45 P.M.	REVIEW OF RECENT DATA/FUTURE PLANS	M. McKUBRE SRI INT'L
2:45 P.M.	BREAK	
3:00 P.M.	DISCUSSION AND QUESTIONS	T. PASSELL EPRI
4:30 P.M.	ADJOURN	

OPENING REMARKS
T.O.PASSELL

- THIS IS A PROJECT REVIEW AT A EARLIER STAGE THAN IS USUAL TO ACCOMODATE THE VISIT OF ADM. WATKINS' COMMITTEE INVESTIGATING THE STATUS OF COLD FUSION RESEARCH.
- THIS ELECTROCHEMICAL CELL RESEARCH WAS INITIATED TO CONFIRM OR REFUTE THE CONNECTION BETWEEN HEAT AND NUCLEAR FUSION EVENTS IN PALLADIUM CATHODES AS REPORTED BY PONS AND FLEISCHMANN
- EVEN THOUGH NO CONCLUSIVE RESULTS ARE YET AVAILABLE FROM THIS EXPERIMENT, I FELT IT WAS USEFUL TO EXPOSE THE CONCEPT AND PROCEDURES BEING FOLLOWED BY THE SRI INT'L RESEARCHERS TO A WIDER AUDIENCE BECAUSE OF THE UNIQUE APPROACH
- THIS INFORMATION IS BY NO MEANS READY FOR PUBLICATION OR THE BASIS FOR ANY CLAIMS THAT COLD FUSION HAS EITHER BEEN CONFIRMED OR REFUTED.

ATTACHMENT 2

Visit of
Cold Fusion Panel
Energy Research Advisory Board

Discussion of Thermal Measurements on Two Fast Mixed - Conductor Systems - Deuterium and Hydrogen in Palladium

Andreas Belzner,
Ulrike Bischler,
Steven Crouch-Baker,
Turgut M. Gür,
George Lucier,
Martha Schreiber, and
Robert A. Huggins

Department of Materials Science and Engineering

Stanford University, Stanford, CA 94305

July 6, 1989

General Outline of Discussion

General Principles - Hydrogen and Deuterium in Palladium

Principles of Isoperibolic Calorimetry
- General Theory
- Possible Issues

Types of Experiments Done at Stanford
- Materials, Cell Designs, Procedures
- Resolution of Possible Issues

Results Obtained

Comments on Materials Science Aspects of Fast Mixed Conductors Such as the D-Pd and H-Pd Systems
- Guest Species are Generally Ionized
- Chemical Diffusion Rates Can Be Extremely High
  - Conventional Site - to - Site Jump Models Cannot Apply
  - Electron Density Distributions Are Unusual
- Large Cell Voltages Can Produce Hot Electrons in Solids

STEVEN CROUCH-BAKER

Research Associate
Department of Materials Science and Engineering
Stanford University
Stanford, CA 94305

Dr. Crouch-Baker obtained his B.A. in Chemistry with first class honors from New College, Oxford University, in 1982, where he was an Open Exhibitioner. He received his M.A. and PhD. in 1985 from the Inorganic Chemistry Laboratory, also at Oxford University. During this time, he was an Honorary Senior Scholar of New College. While at Oxford, he was a mathematics tutor at New College and, during his postgraduate studies, a Junior Demonstrator in the Inorganic Chemistry Laboratory, where he was responsible for the organization and supervision of undergraduate practical work. In 1985, he joined the Solid State Ionics Laboratory of the Department of Materials Science and Engineering at Stanford University, under the direction of Professor Robert A. Huggins, as a Post-Doctoral Research Affiliate. Since October, 1987, he has been a Research Associate with the Laboratory. In addition, he is currently a Lecturer at the University of California at Los Angeles, where he teaches a graduate level course in advanced thermodynamics. He has served as a consultant to the Solar Energy Research Institute in Boulder, Colorado and to Southwall Technologies in Palo Alto, California.

Dr. Crouch-Baker's current research includes successful comparative thermal measurements of "solid-state fusion" in electrochemical cells. Other research activities in solid state ionics have emphasized the electrochemical investigation of phase behavior in solids. These include the study of materials for mixed-conducting electrodes for use in advanced lithium- and sodium-based battery systems; the application of atmospheric pressure chemical vapor deposition techniques to the fabrication of thin-film battery components; the design of new solid hydroxide ion conductors for use in water elecltrolysis/fuel cell systems; and the study of electrochromic compounds for use in applications such as smart windows. Previous work in solid state chemistry has included the synthesis and characterization of several transition metal oxide phases.

He is the author or co-author of over 25 publications concerning these topics.

Dr. Crouch-Baker is a member of the American Chemical Society, the Royal Society of Chemistry, the Materials Research Society, and the Electrochemical Society.

TURGUT M. GÜR

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Dr. Gür received his B.Sc. and M.Sc. in Chemical Engineering from Middle East Technical University in Ankara, Turkey, and his PhD. "with distinction" in Materials Science and Engineering from Stanford University. He joined the Department of Chemical Engineering of Middle East Technical University as Assistant Professor and taught for more than three years. Upon his return to the United States, he worked both in academic and industrial research laboratories including Stanford University, the Energy Systems Laboratory at Continental Group, Inc. and at Raychem Corporation. He rejoined the Solid State Ionics Laboratory at Stanford University, under the direction of Professor Robert A. Huggins, in 1987 as a Senior Research Associate.

He was awarded the I.B.M. Post-Doctoral Research Fellowship (1987-1988), the A. I. D. Junior Fellowship (1970-1974) and the Petkim Petrochemical Industries Scholarship (1963-1966). He earned the academic title of Dozent (associate professor) from the Council of Higher Education of Turkey in 1987. He is also listed in WHO's WHO in the Frontiers of Science and Technology (2nd ed.) and WHO's WHO in the World (8th ed.).

His current research includes successful comparative thermal measurements of "solid state fusion" in electrochemical cells. His other research areas have included heterogeneous catalysis, high temperature electrosynthesis, fast ion conducting solids, gas separation membranes, chemical and humidity sensors, sol-gel and chemical vapor deposition techniques for electro-ceramics, and high Tc superconducting oxides.

He is a member of the Materials Research Society, American Chemical Society, the Electrochemical Society, and the American Ceramic Society.

He is the author or co-author of over 25 publications concerning these topics.

MARTHA SCHREIBER

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Dr. Schreiber was born in Linz, Austria. Her early education included secondary school, and later, study at the Musisch pædagogisches Realgymnasium in Vienna, Austria.

Her university work (1973-1980) centered upon the study of chemistry at the Technische Universität Wien (Technical University of Vienna), where she specialized in inorganic chemistry. During 1976-1978 she was employed as a technician at the Institute of Analytical Chemistry at the Technische Universität Wien. Her Diplomarbeit (1979-1980) was entitled "Anodische Auflösung von Aluminium in wässrigen chloridhaltigen Elektrolyten" ("Anodic Dissolution of Aluminum in Aqueous Chloride-Containing Electrolytes"), and was completed under the supervision of Professor Christoph Fabjan. During 1980-1981 she was a Member of the Technical Staff at the Hauptpunzierungsamt Wien I.

During 1981-1983 she undertook research and prepared a doctoral thesis at the Institut für chemische Technologie anorganischer Stoffe (Institute for Inorganic Chemical Technology) of the Technische Universität Wien under the guidance of Professor Benno Lux and Professor Werner Wruss. Her thesis was entitled "Beitrag zur Entwicklung einer Schneidkeramik auf der Basis von Aluminiumoxid mit Zusätzen von Titannitrid, Titancarbonitrid, Titancarboxinitrid und Zirkonnitrid" ("Contribution to the Development of a Cutting Tool Ceramic Based Upon Aluminum Oxide, With Additions of Titanium Nitride, Titanium Carbonitride, Titanium Carbooxynitride, and Zirconium Nitride") and was aimed at the improvement of the mechanical properties of sintered alumina by adding TiN, TiO, TiC, and ZrN for use in cutting tools. She received the degree "Doktor der Technischen Wissenschaften" (Doctor of Applied Science) with distinction in June, 1983.

From March, 1984 to February, 1987 she held the position of Universitäts-assistent at the Institut für Technische Elektrochemie at the Technische Universität Wien. There she was involved in teaching, research, and administration. Her research was in the area of solid state chemistry and solid electrolytes, particularly investigations of ß"-alumina solid electrolytes and their isomorphs.

In February 1987, Dr. Schreiber was awarded an "Erwin Schroedinger Fellowship" by the Austrian National Science Foundation to undertake research with Professor R. A. Huggins in the Department of Materials Science and Engineering at Stanford University. She studied the thermodynamic and kinetic properties of metal hydrides based on Ti-Ni intermetallic phases, for hydrogen storage materials, as well as for use as negative electrodes in rechargeable battery systems. While at Stanford, first as a Visiting Scholar, and subsequently as a Research Associate, Dr. Schreiber has also assumed major responsibility for three additional research programs: a novel method for the dissociation of water to produce hydrogen, involving an intermediate temperature electrochemical hydrogen pump containing hydrogen-transparent metal membranes; an approach to a high efficiency direct energy conversion device based upon a hydrogen-transporting electrochemical system; and the investigation of an elevated temperature molten salt electrochemical method for the synthesis of refractory intermetallic compounds. Her current research includes successful comparative thermal measurements of "solid-state fusion" in electrochemical cells.

She is the author or co-author of 13 publications concerning these topics.

ROBERT A. HUGGINS

Professor
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Professor Huggins obtained his B.A. in Physics from Amherst College and his M.S. and Sc.D. in Metallurgy from the Massachusetts Institute of Technology. After serving as an Instructor at MIT, he joined the Stanford faculty in 1954. Professor Huggins was the Director of Stanford's Center for Materials Research from its initiation in 1961 until 1977.

He was awarded the Hardy Gold Medal of the AIME in 1957 and was selected as one of the Five Outstanding Young Men of California in 1963. He held a National Science Foundation Senior Postdoctoral Fellowship in 1965-66 at the Max-Planck-Institut fuer physikalische Chemie in Goettingen, Germany. During 1968-70 he served as Director of Materials Sciences in the Advanced Research Projects Agency in Washington, D.C. He received a 1977 Alexander von Humboldt Foundation Senior United States Scientist Award to pursue research at the Max-Planck-Institut fuer Festkoerperforschung, in Stuttgart, Germany. He was presented the 23rd Annual Vincent Bendix Award by the American Society for Engineering Education in 1978, and was named a Case Centennial Scholar by the Case-Western Reserve University in 1980. He received the Research Award of the Electrochemical Society's Battery Division in 1982.

His research activities have included studies of imperfections in crystals, solid state kinetics, magnetism, mechanical behavior of solids, crystal growth, and solid state electrochemistry. Primary attention has recently been focused on solid state ionic phenomena involving solid electrolytes and mixed ionic-electronic conducting materials containing atomic or ionic species with unusually high mobility, as well as their use in novel battery and fuel cell systems, electrochromic optical devices, sensors, and in enhanced heterogeneous catalysis. A topic of particular interest at the present time is hydrogen transport and hydride formation in metals, alloys and intermetallic compounds.

He is author or co-author of over 240 publications, including articles or chapters in 23 books, concerning these and other fields. He is Editor of the series Annual Review of Materials Science, Editor of Solid State Ionics, Associate Editor of the Materials Research Bulletin, on the Advisory Review Board of the Journal of Materials Research, and on the Editorial Boards of Progress in Solid State Chemistry, Semiconductors and Insulators, and the European Journal of Solid State and Inorganic Chemistry.

Professor Huggins served as Chairman of the Solid State Sciences Committee, and was a member of the Committee on Advanced Energy Storage Systems and the Committee on Battery Materials Technology of the National Academy of Sciences-National Research Council, as well as being a member of numerous other technical and governmental committees.He was Chairman of the 5th International Conference on Solid State Ionics, and is currently a Councillor of the Materials Research Society, and President of the International Society for Solid State Ionics.

Major Quandaries

Existence Question

Is anything unusual actually happening when deuterium is inserted into palladium at very high activities ?

Reproducibility Question

Why do some people observe effects and some evidently do not ?

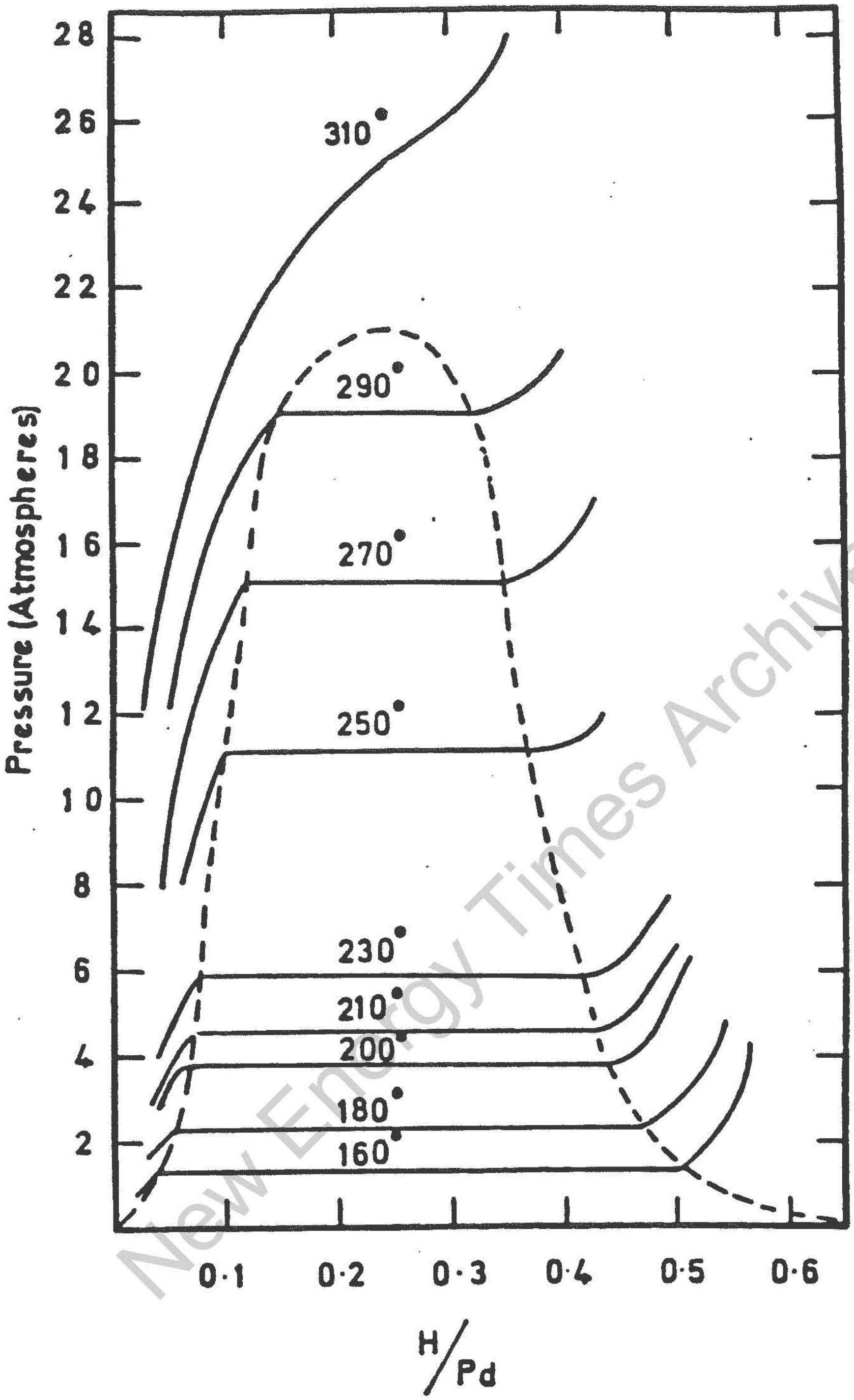
Why so some samples seem to show effects, and others do not - even in the same laboratory ?

Mechanism Question

What is causing the observed effects to occur ?

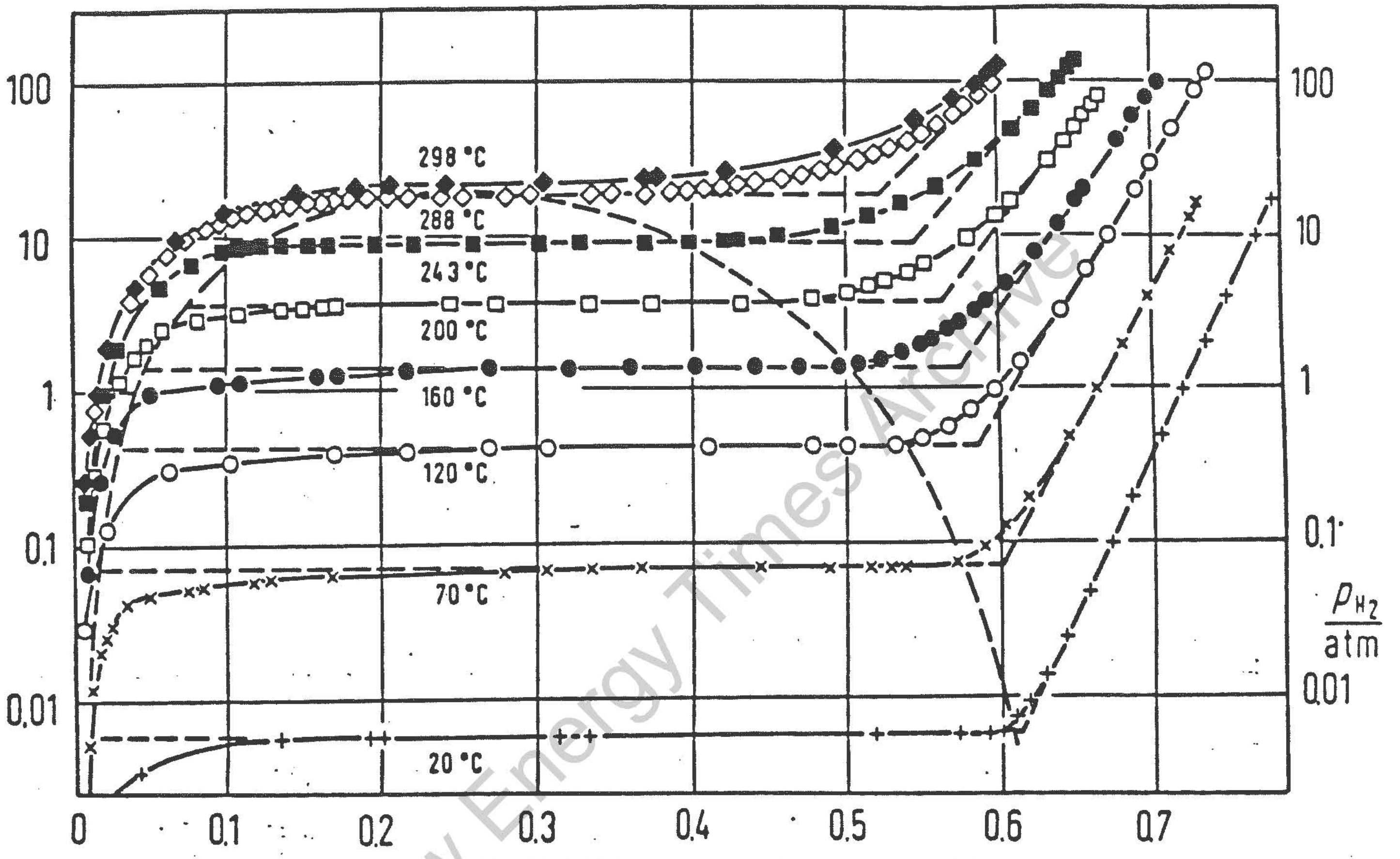
What causes the observed time dependence ?

Other experimental parameters ?

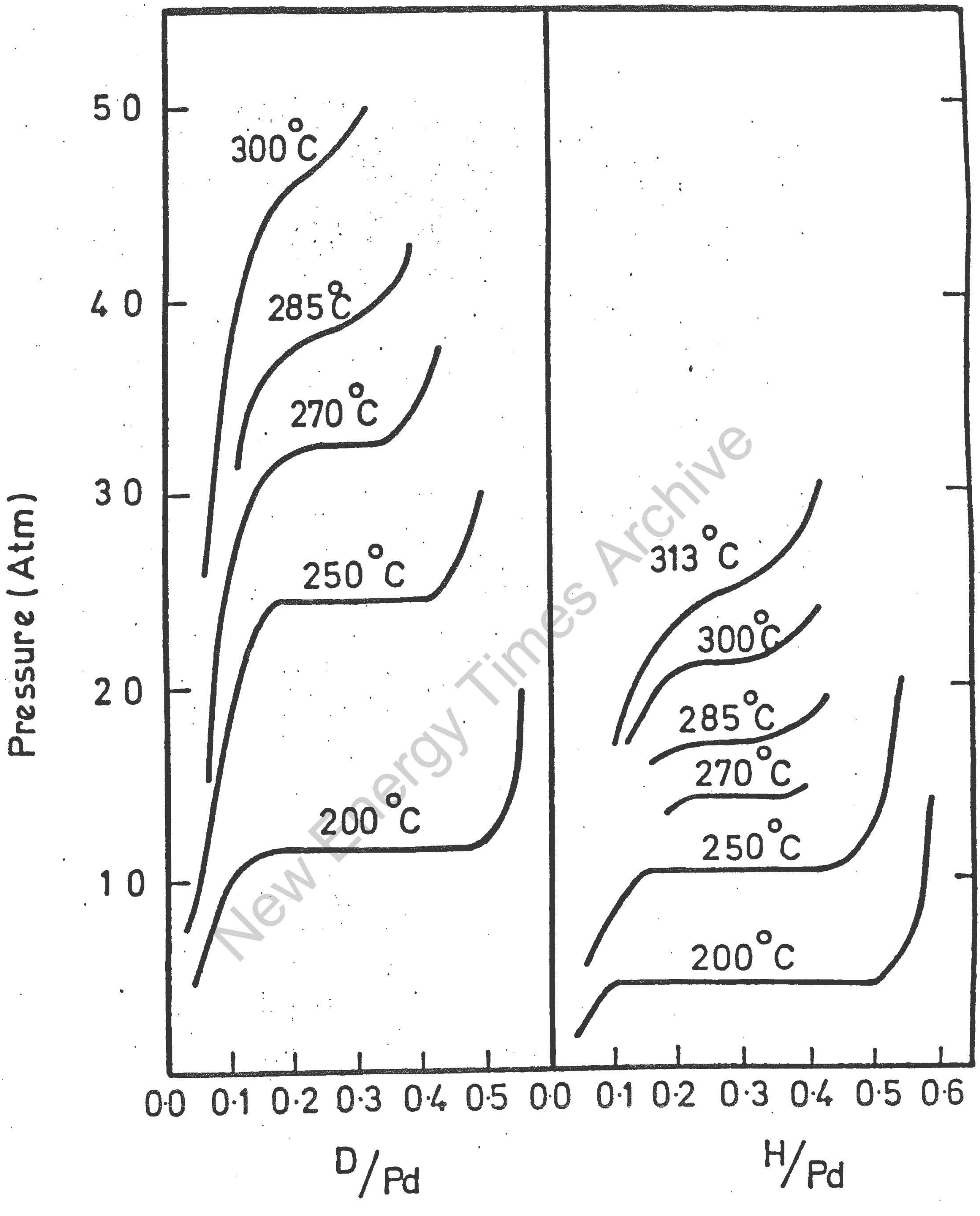


p/c Isotherms in the Pd-H System

H. Bruening and A. Sieverts, Z. Phys. Chem. 163A, 409 (1933)



p(*n*) isotherms of Pd(H) with bulk palladium at different temperatures.



Pressure (Atm)
5 0
4 0
3 0
2 0
1 0
300°C
285°C
270°C
250°C
200°C
313°C
300°C
285°C
270°C
250°C
200°C
0·0
0·1
0·2
0·3
0·4
0·5
0·0
0·1
0·2
0·3
0·4
0·5
0·6
D/Pd
H/Pd

Stanford Work

Experiments to determine whether there is a significant difference in thermal effects when deuterium, rather than hydrogen, is electrochemically inserted into palladium.

Experiments were undertaken to directly measure heat effects using conventional calorimetric methods.

Direct comparison between the behavior of the deuterium-palladium and hydrogen-palladium systems under experimental conditions as comparable as possible.

To balance out the heats of solution in the α and β phases, the enthalpy of the α-β phase transformation, interfacial or external chemical reactions or any other extraneous effects.

Except for neutron and gamma ray monitors used for safety purposes, no radiation detection measurements were undertaken.

For any given value of heat input (the sum of electrical Joule heating and any internal heat generation mechanism) each cell reached a steady state temperature, such that the total heat input was just balanced by the heat loss through the cell walls into the surrounding cold water bath. Thus for the same value of electrical power input, any difference in internal heating of these cells would result in a difference in their steady state temperatures.

Potential differences were applied in 1 V increments from 3 V up to 15 V. The observed current values were between 10 and 1000 mA. The maximum temperature difference between the cells and the external bath was about 10 °C. Data points were taken under both increasing and decreasing cell voltages to assure that no hysteresis effects were present.

Simple tests were carried out to assure that significant systematic errors were not present in the experimental arrangement.

Principle of Isoperibolic Calorimeter

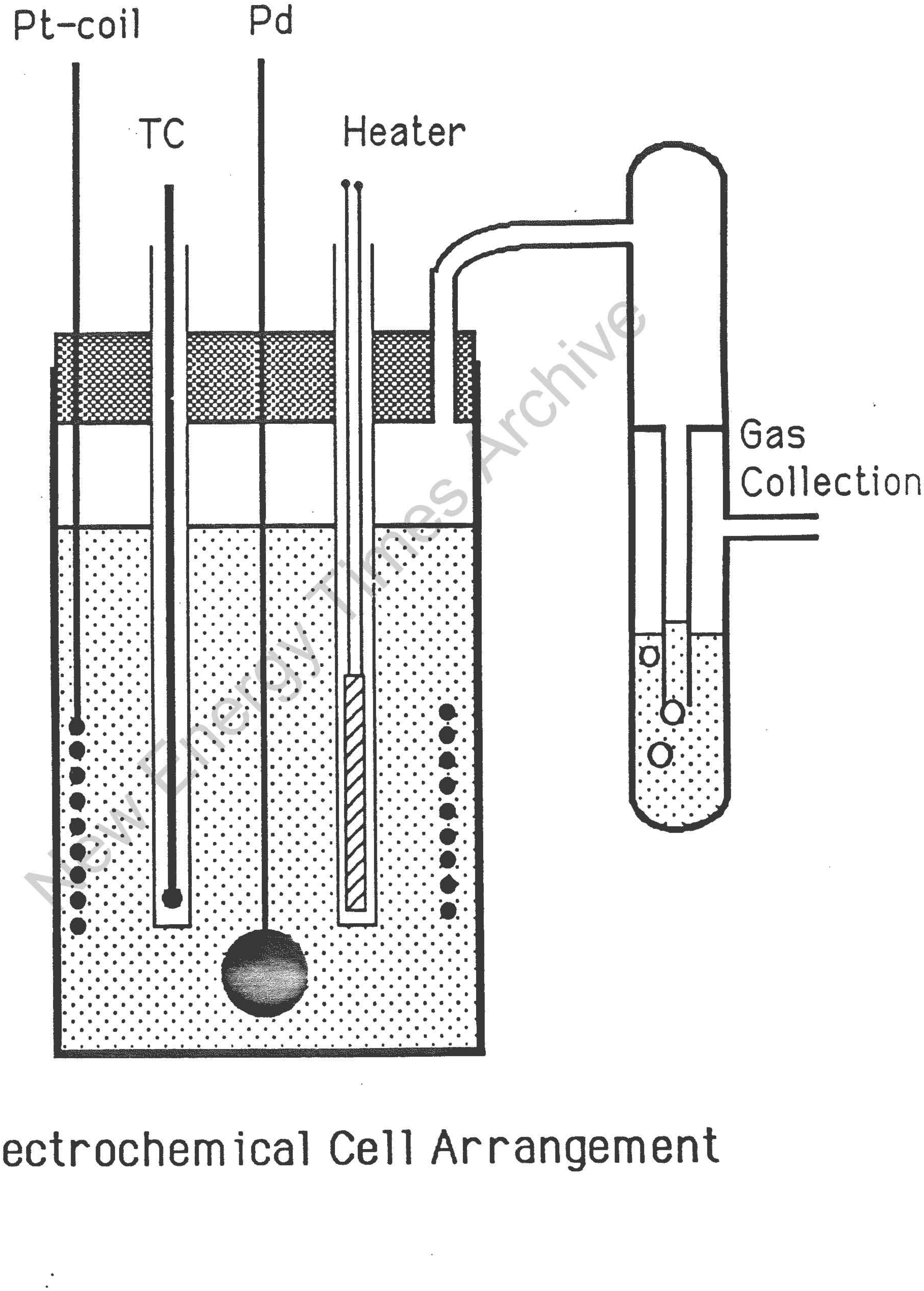
Dynamic power balance under steady state conditions

Electrical power supplied, plus power generated/consumed within cell = Power output

Temperature difference between cell and surrounding cold bath is proportional to the total power output from the cell.

$$\text{Power output} = K\ (T - T_{bath})$$

K = calibration constant



Electrochemical Cell Arrangement

Issues Related to Possible Errors

Insufficient Stirring

Calibration Variation With Amount of Stirring

What Do We Do ?

1)Introduce additional mechanical stirring

Gyrotary Water Bath Shaker - New Brunswick Scientific

Measure T - Tb as function of stirring

Operate in range where additional stirring has no effect

2) Recent data includes heater calibration of cell constant three times at each electrochemical power level

What About Other Effects ?

Endothermic Phenomena

Heat leakage from cell

  Up wires, tubing, connectors, etc.

Sensible heat carried out of cell by gases

Evaporation of the solvent (H_2O or D_2O)

These all lead to reduced cell temperature

Thus any heating effects are larger than observed

Exothermic Phenomena

Insertion reaction of H or D into palladium

Recombination of H_2 (D_2) and O_2 gases

Any corrosion reactions

Any subsequent reaction of gaseous products

Mechanical energy input from stirring

Note that no exothermic effects related to the primary cell reaction can be larger than that reaction's endothermic effect.

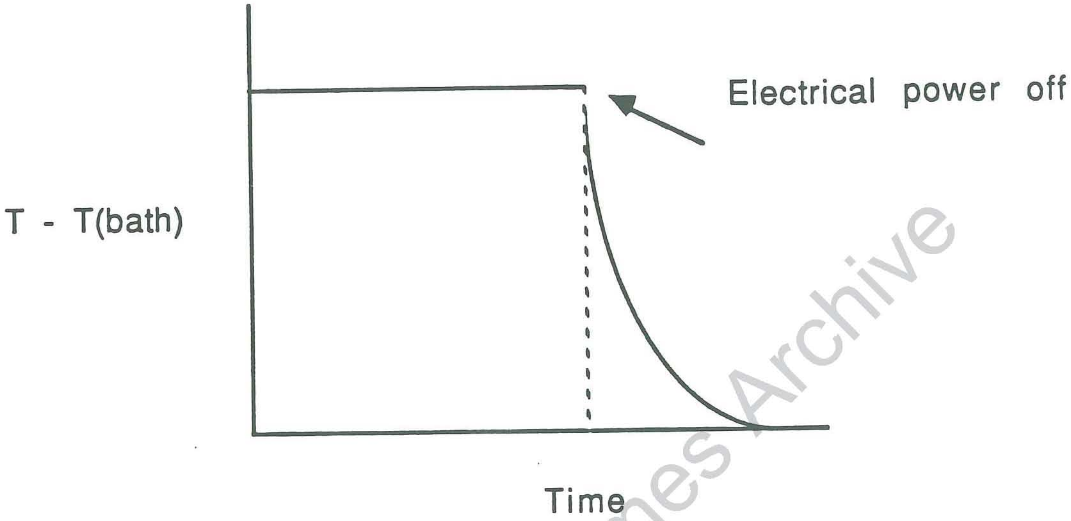
For example, any heat from recombination cannot be greater than the heat absorbed by electrolysis

It is impossible to get excess heat (greater than from simple resistor) from any such reaction

Other observations:

1. If turn off electrical input -

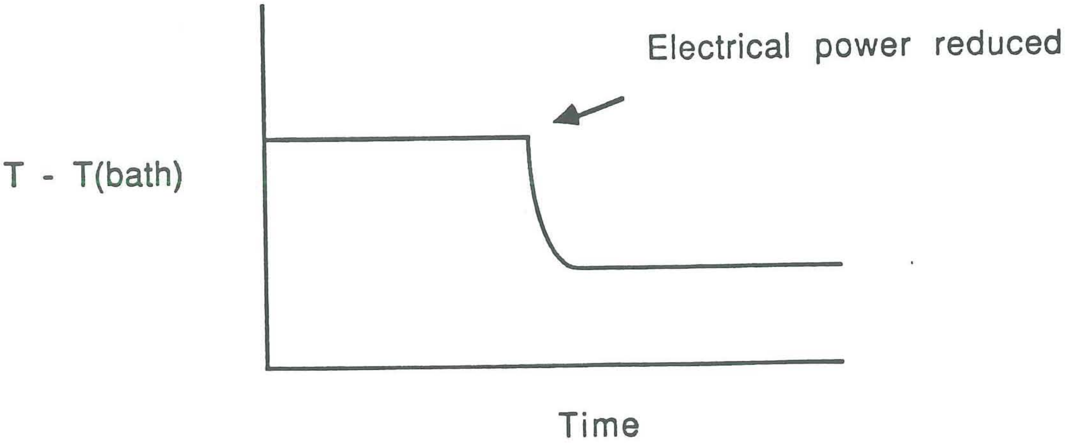
a. Heat effect stops.



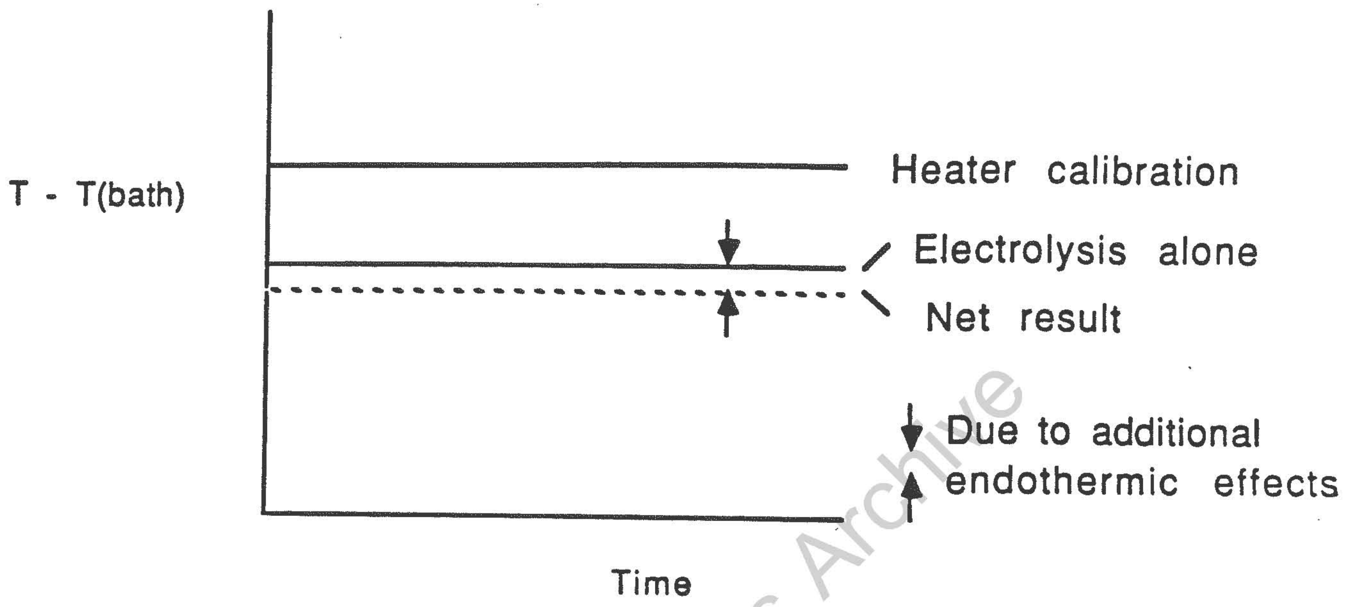
b. Copious gas evolution from Pd.

2. If reduce electrical input -

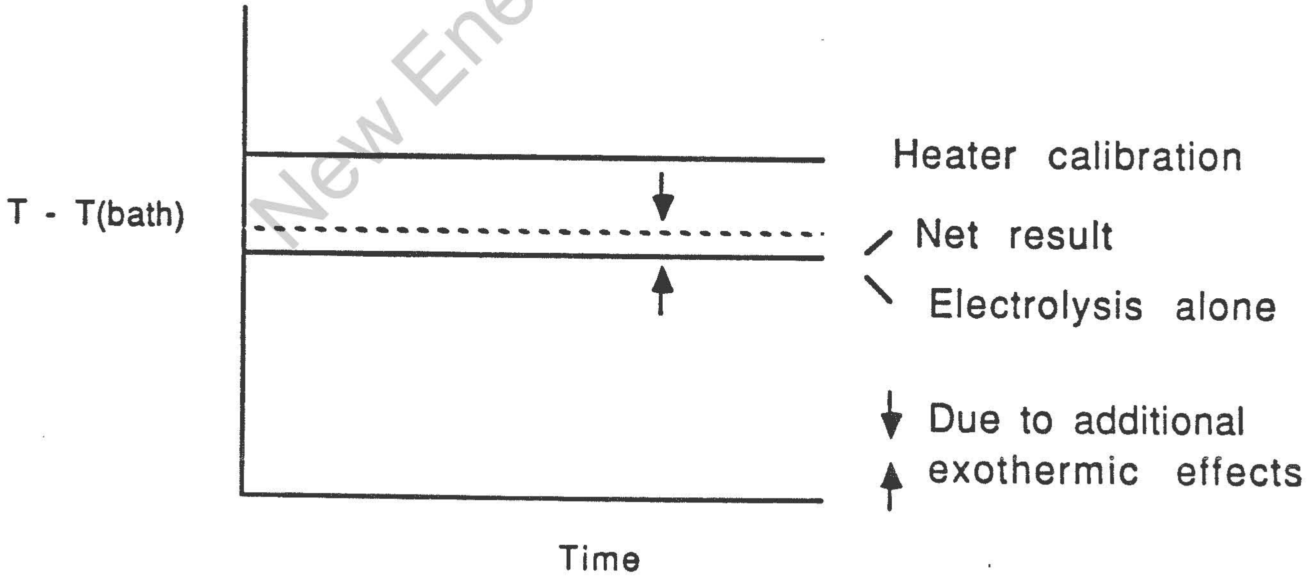
a. Cell temperature drops to lower value.



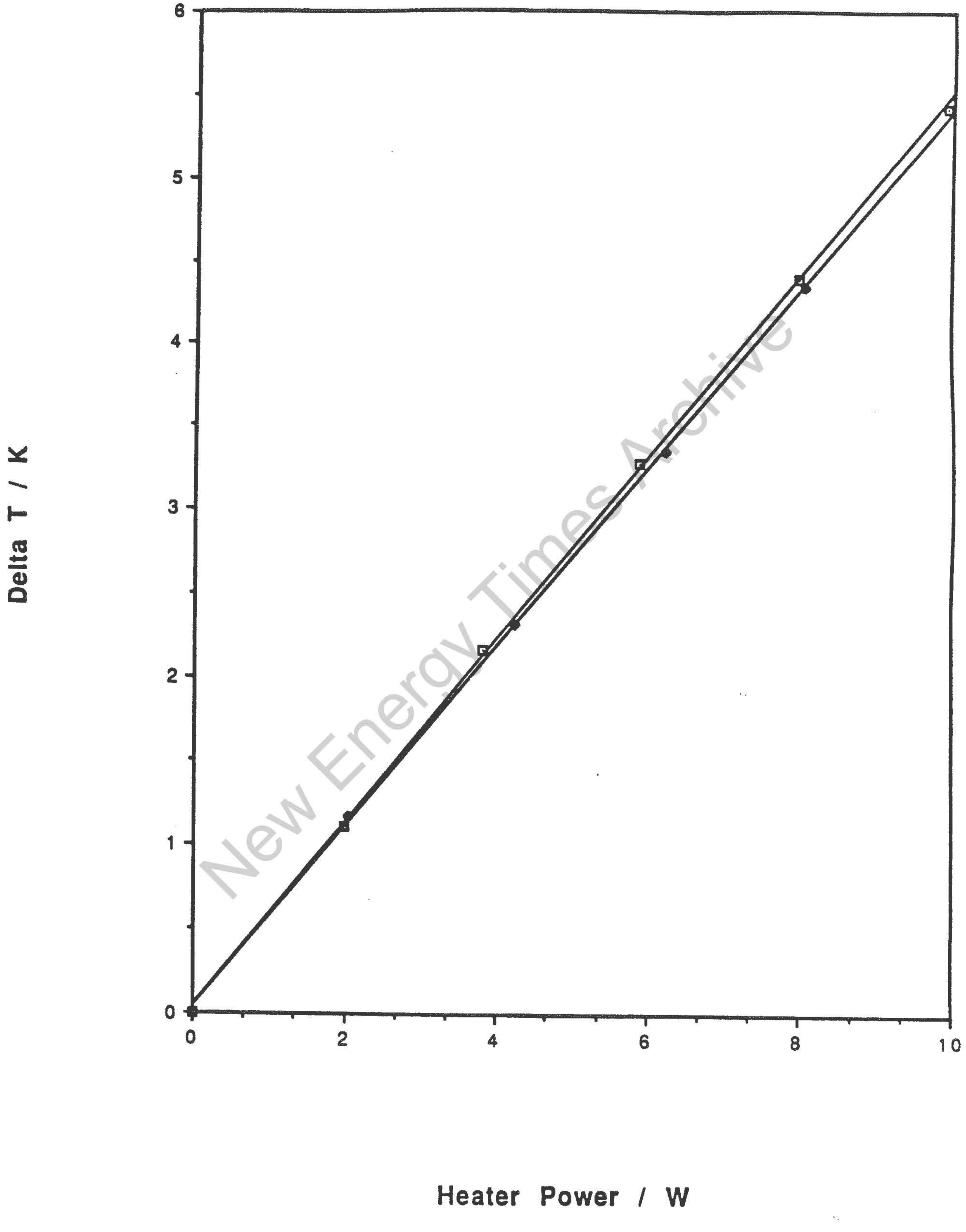
Endothermic effects.



Exothermic effects.



Heater Calibration - D Cell, H Cell

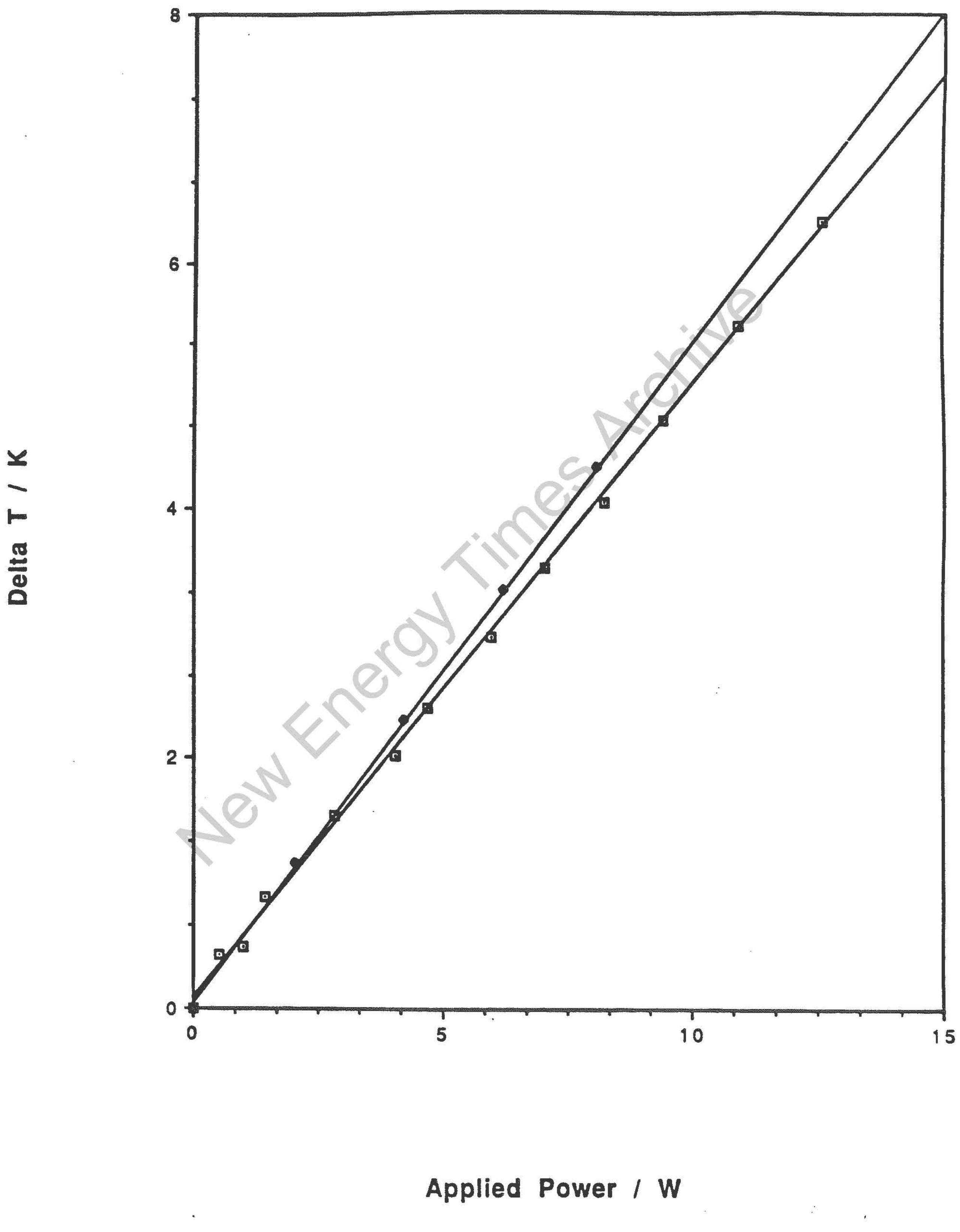


Measurements of Endothermic Effect Due to Electrolysis

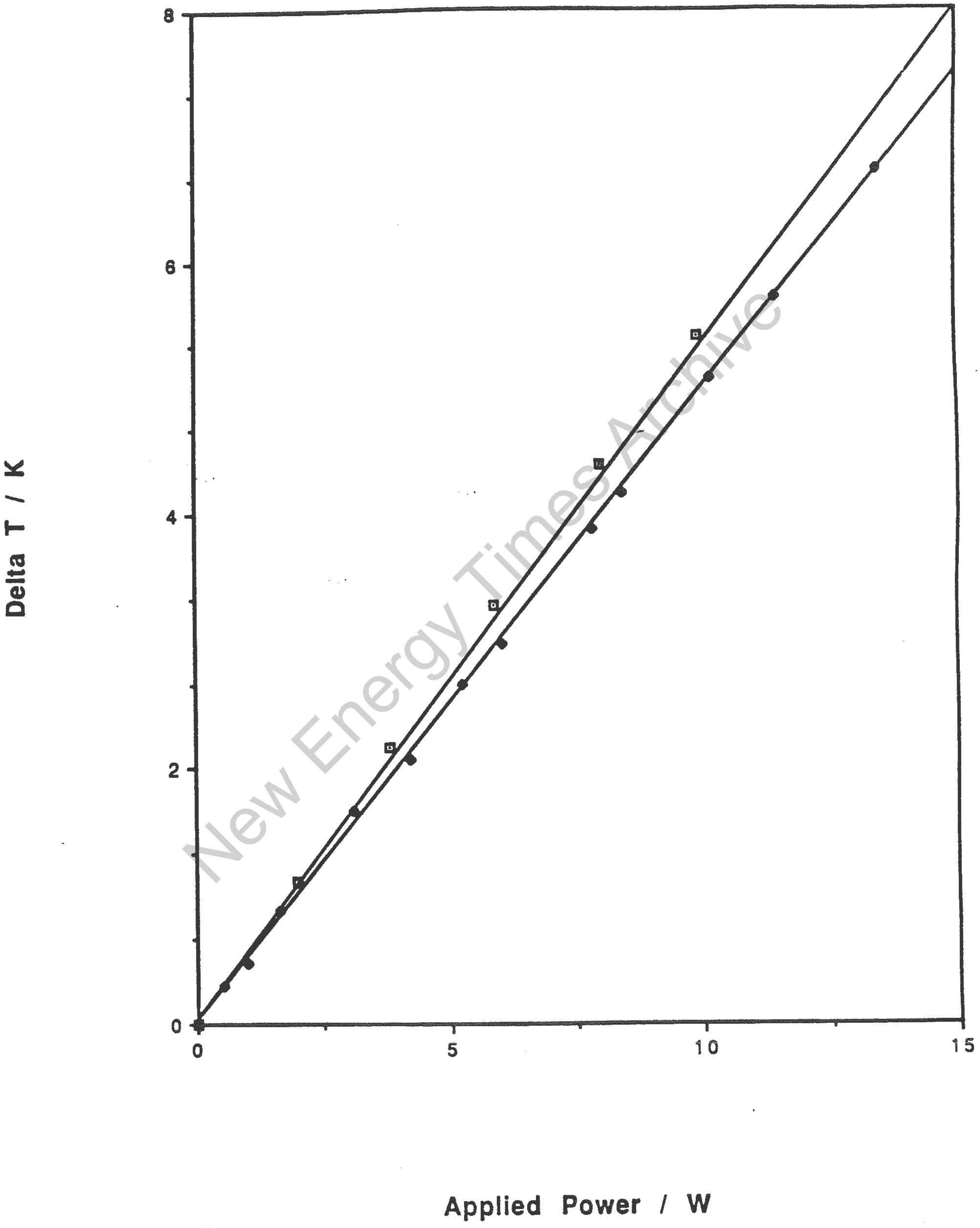
Thermal results corresponded to within 93 % of the theoretical value

This means that all other exothermic processes did not contribute more than 7 % of the total measured temperature rise.

H - Cell "Zero Time" vs. Calibration

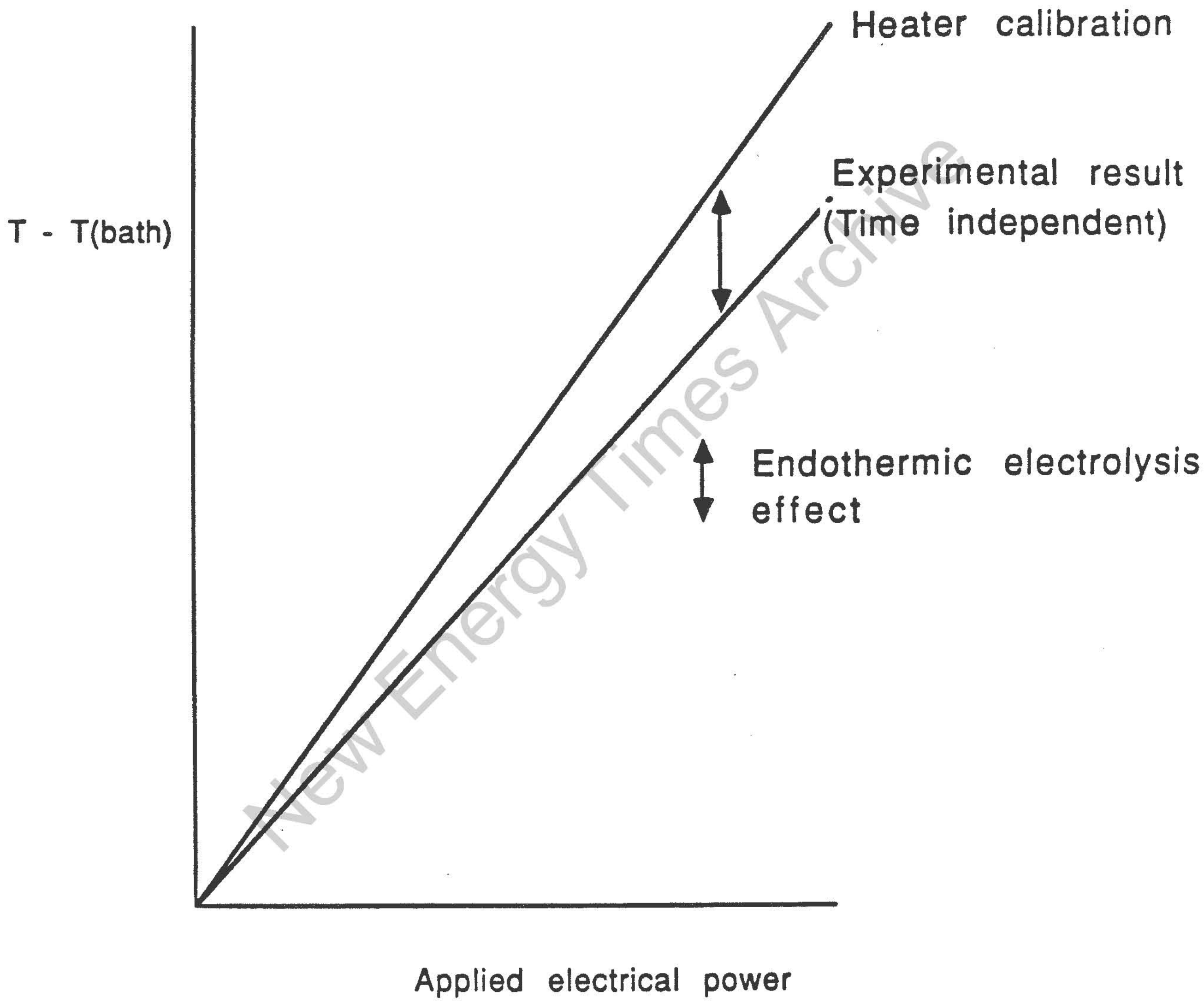


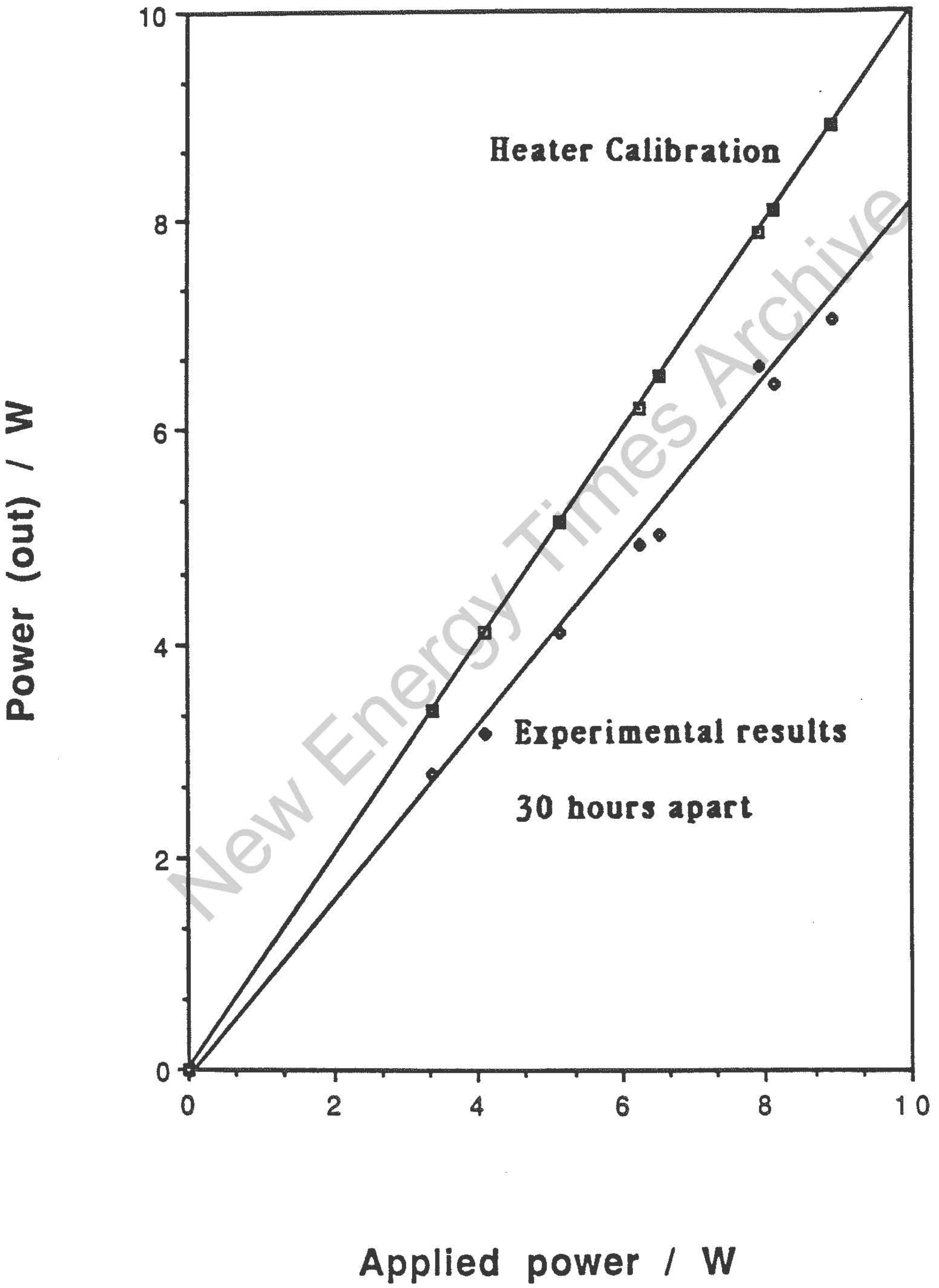
D - Cell "Zero Time" vs. Calibration



What do we actually observe ?

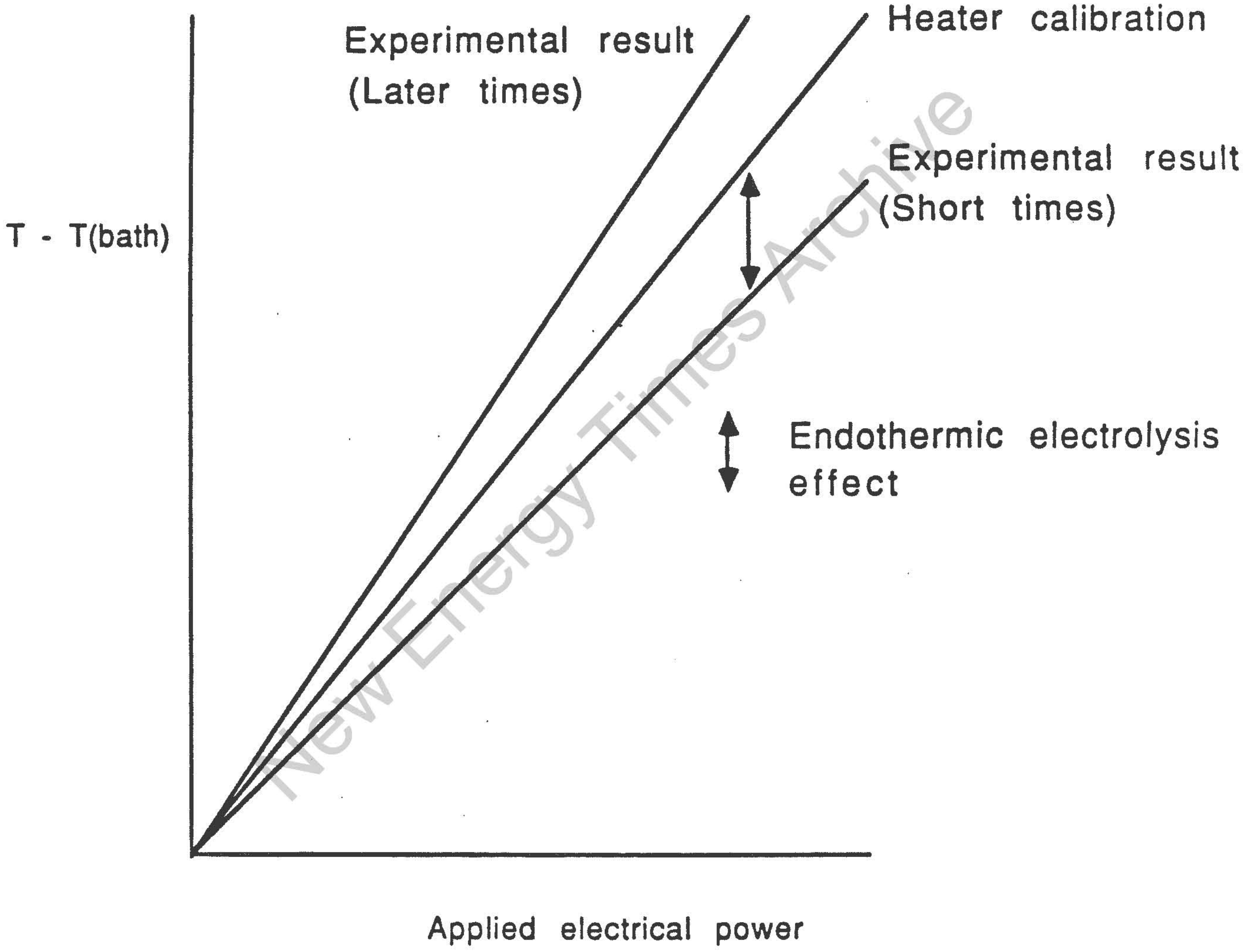
1. Light water



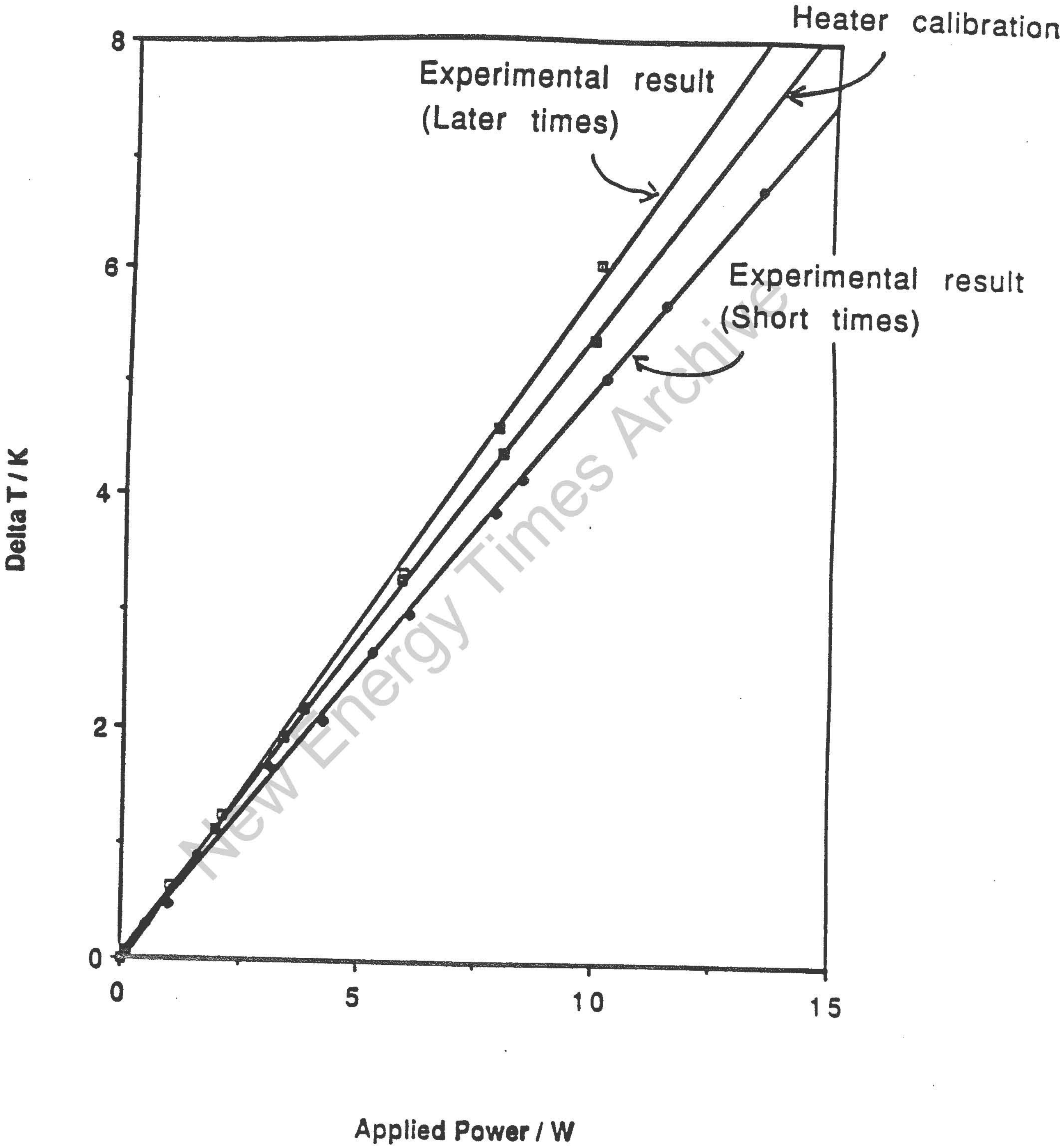


H - cell endothermic effect vs. time

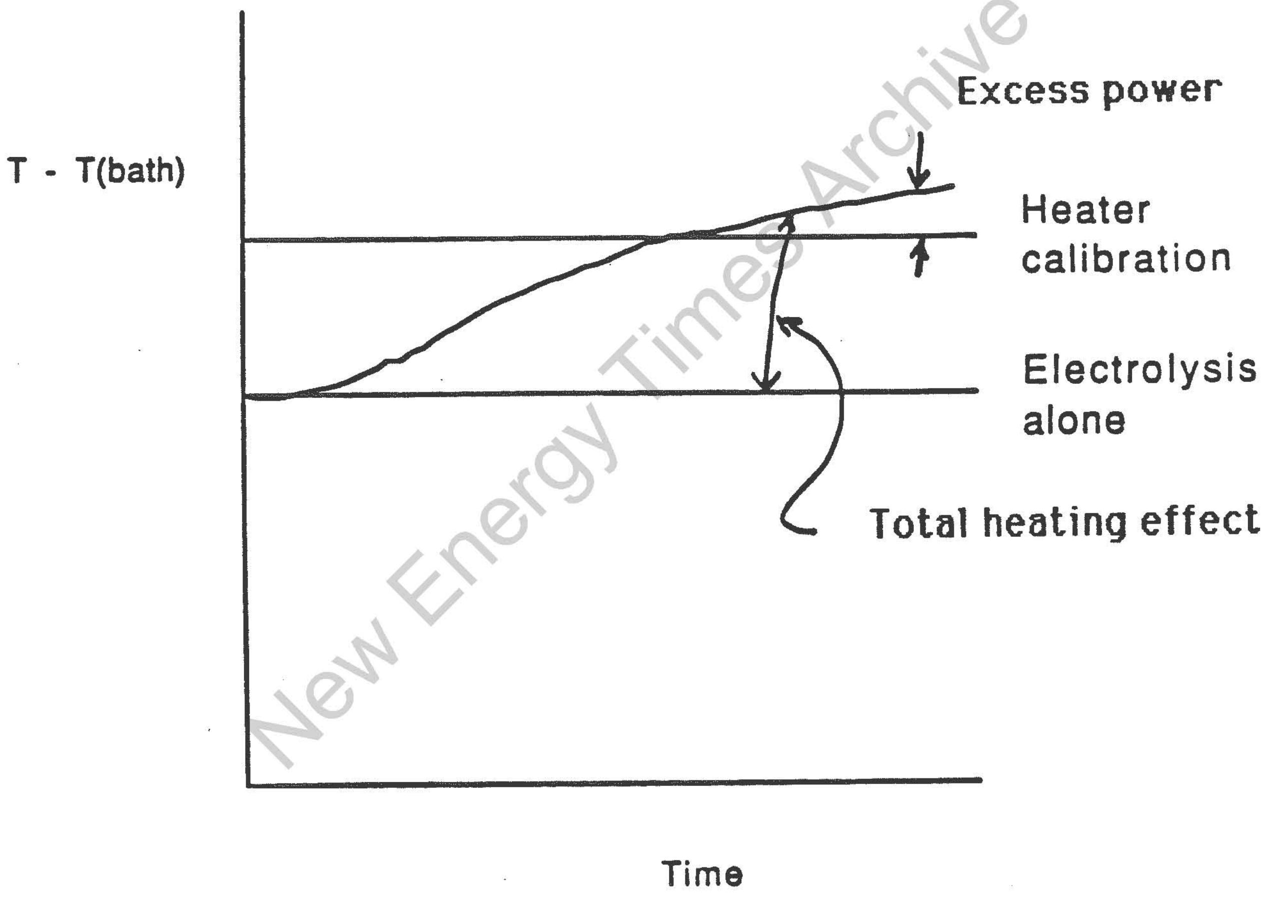
2. Heavy water



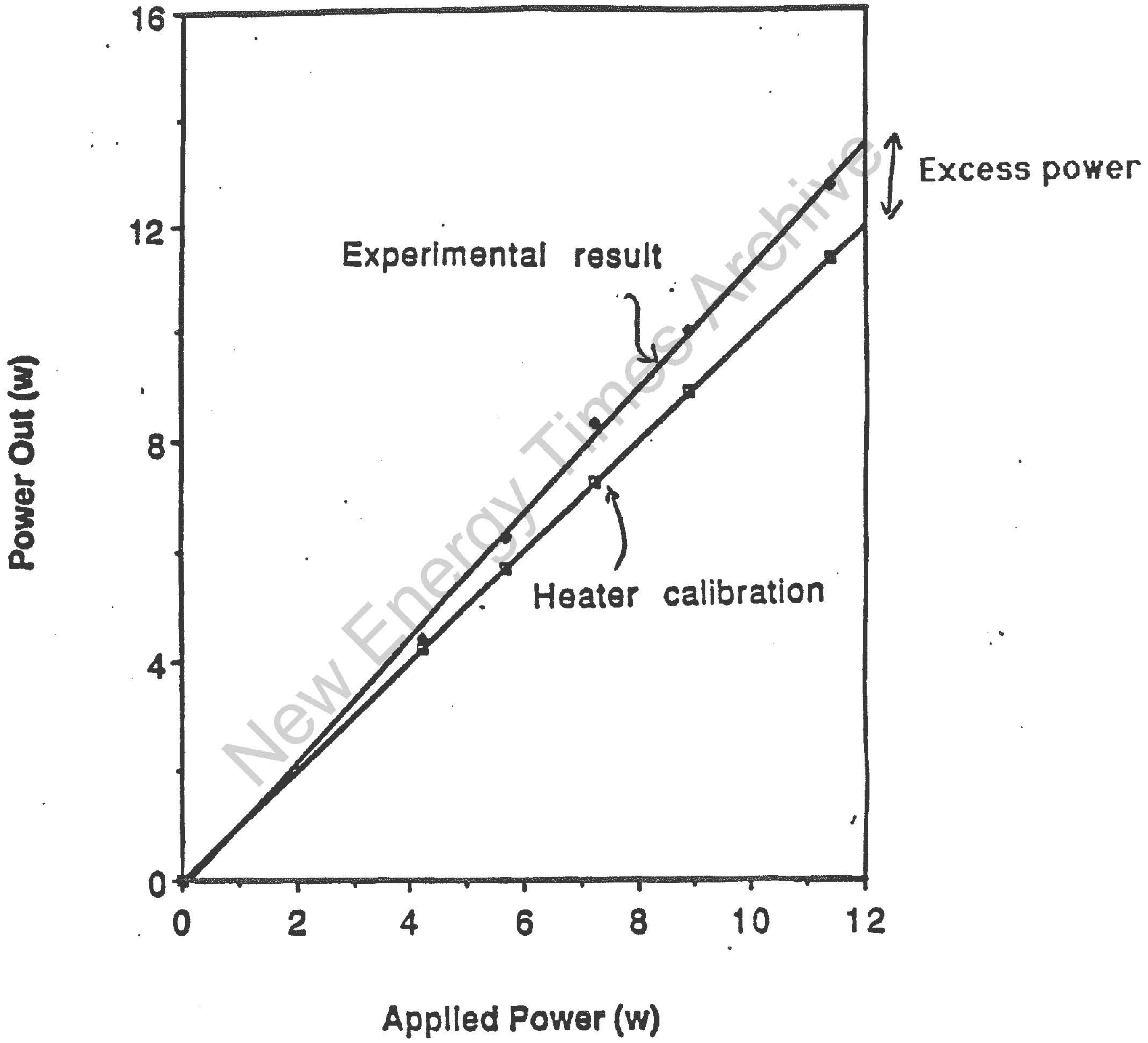
Measurement of heat effect in D - cell



3. Time dependence.



Power Scan for D-cell After 5 Days (5/23/89)



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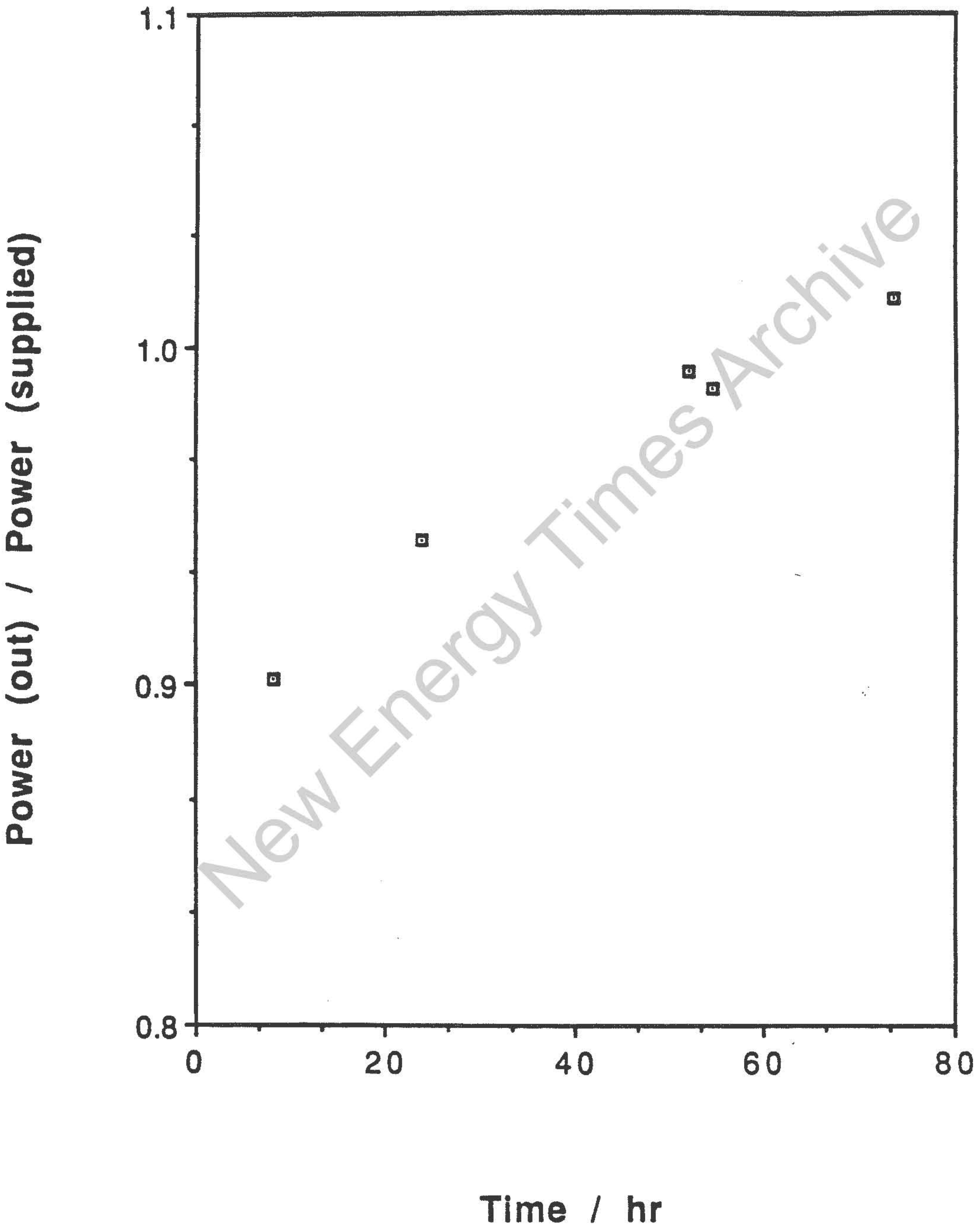
Time dependence of heat effect in D - cell



What Do We Actually Observe ?

H_2O Case

Cell temperature lower with electrolysis than with calibration heater

With electrolysis, cell temperature is independent of time

D_2O Case

Cell temperature with electrolysis is initially lower than calibration heater

With electrolysis, cell temperature gradually rises with time

Reaches values greater than with calibration heater.

This is "excess power".

Excess power must be due to a phenomenon not related to basic electrolysis reaction

Cannot be due to recombination of D_2 and O_2

Must be some phenomenon that is time - dependent

Implies that it happens inside solid, not on surface

Important Experimental Considerations

1. Microstructure and interstitial content of the palladium

2. Presence of other impurities

3. Heavy water is unstable in the presence of light water.

1. Microstructure and interstitial content of the palladium

Cast versus wrought (drawn or extruded) samples

Is the dislocation structure important ?

Dislocations are well known to be traps for hydrogen

Outgassing

Removal of species which competitively occupy interstitial sites - e.g. hydrogen and oxygen

We repeatedly arc melted our palladium in order to reduce the initial concentration of hydrogen and other volatile impurities.

The presence or absence of such species is readily apparent from visual observation of the optical emission of the arc.

Others who have use melted (cast) materials have been successful in observing large effects.

Evidence for Blocking Layers on Palladium Surface

When turn off electrical input

See no gas evolution from surface

Observe large rise in cell resistance

xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx

There have been reports of finding platinum on the surface of the palladium when platinum positive electrode was used at high currents

There have been reports of finding silicates on the surface of the palladium - probably related to the attack of the glass container.

3. Heavy water is unstable in the presence of light water.

It is well known that heavy water will react irreversibly with light water (e.g. in the atmosphere) to form HDO.

Hydrogen, rather than deuterium, is preferentially evolved during the electrolysis of a D_2O / HDO / H_2O mixture. This is the basis for the use of electrolytic methods to concentrate D_2O.

Likewise, hydrogen, rather than deuterium, is preferentially absorbed into palladium.

Thus the presence of hydrogen - containing impurities in the heavy water - based electrolyte will lead to the formation of hydrogenated, rather than deuterated, palladium.

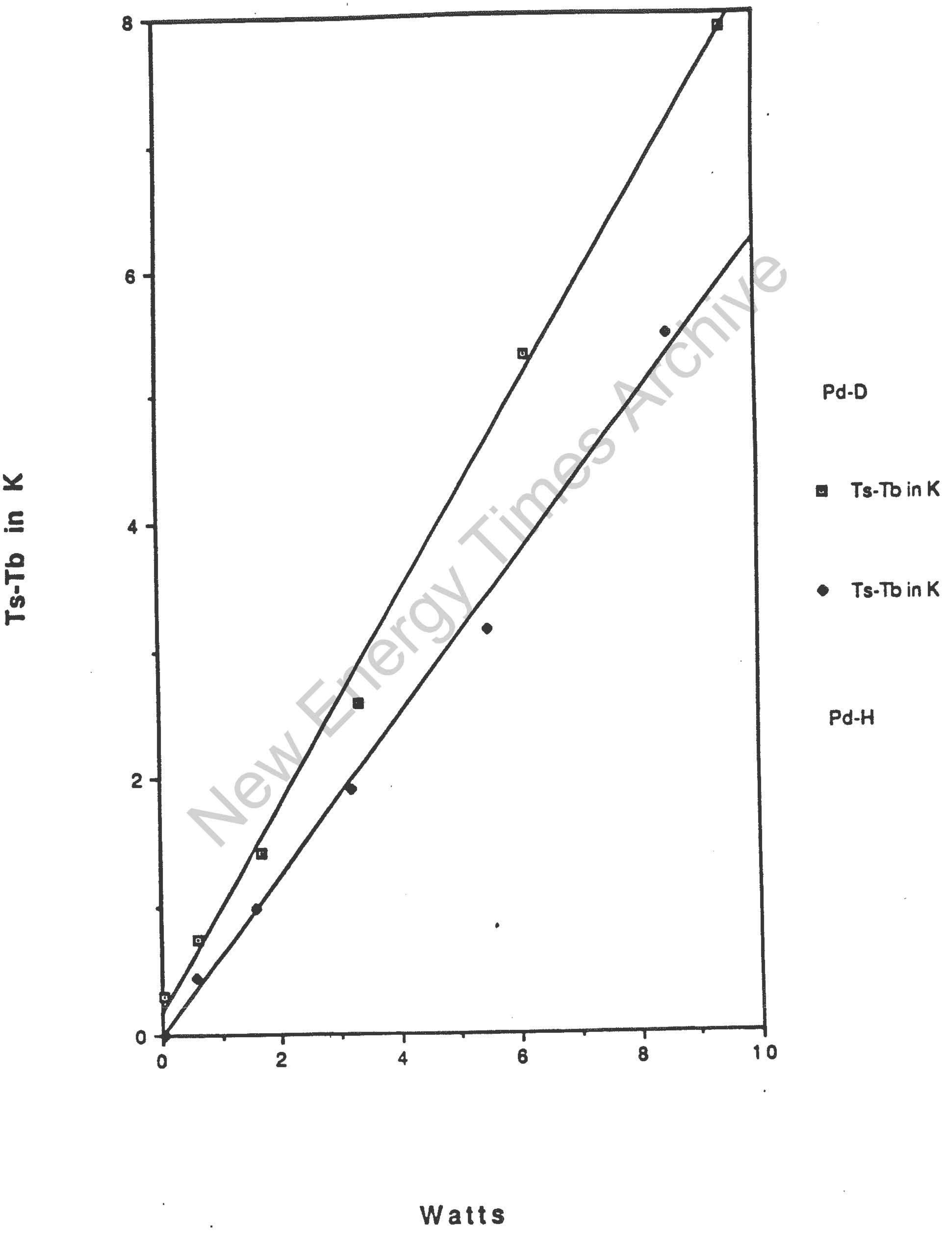
Consequently, care must be taken at all times to prevent the cell contents from coming into contact with atmospheric moisture, both during cell assembly and subsequent operation.

Evidence for Influence of Light Water Incorporation Into Heavy Water

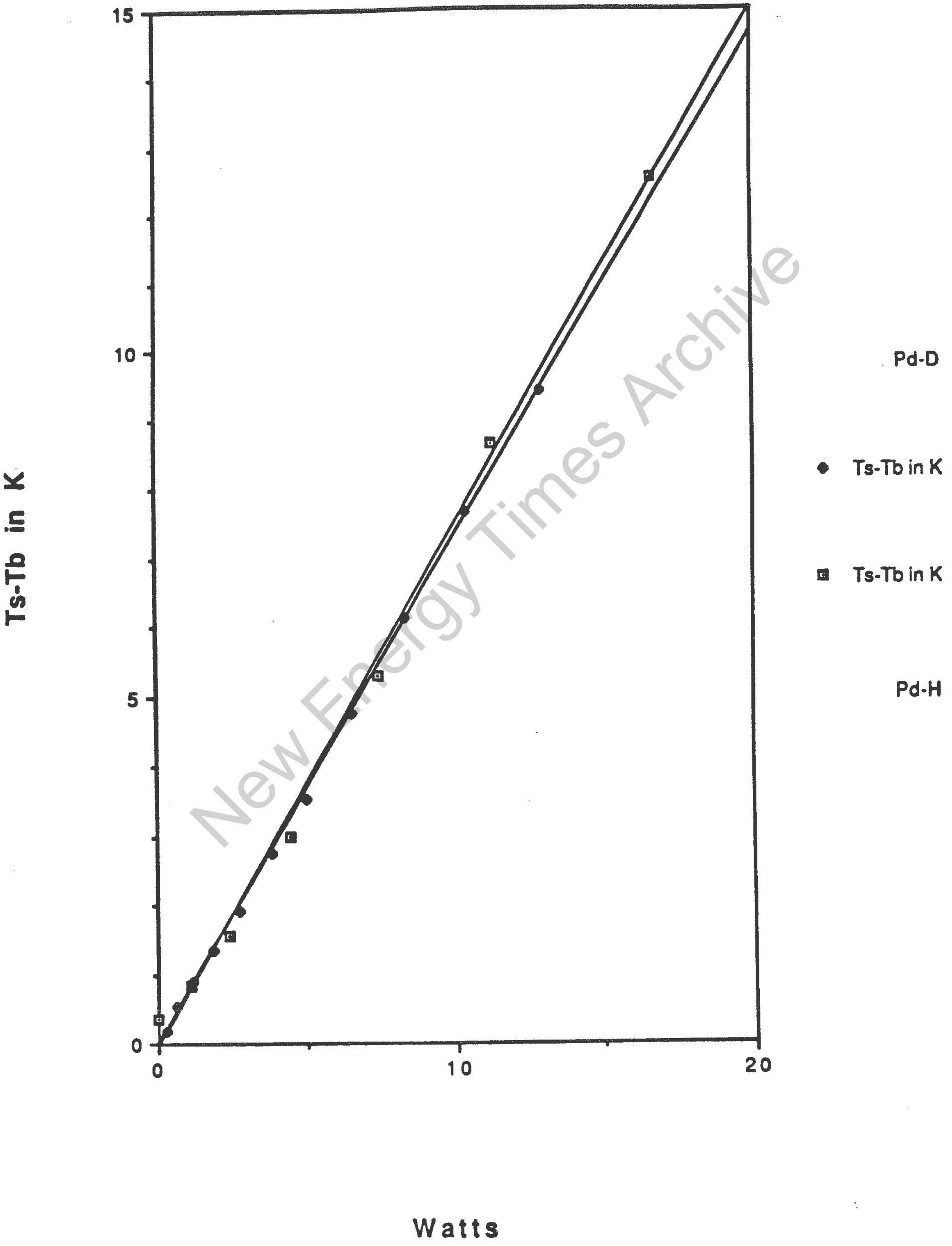
Decay of excess heat with time after exposure to moist atmosphere

Infra-red spectrum showed large concentrations (20 - 30 %) of both H_2O and HDO

After Precharging



After Exposure to Air For 1 Day



Major Results of the Stanford Experiments

In electrochemical experiments involving the insertion of hydrogen and deuterium into palladium, the thermal behavior of the two systems is consistently different.

Whereas both systems show the expected endothermal effect due to electrolysis, there is some process that produces excess power in the D - Pd case.

This excess power production appears only after deuterium has been inserted into the palladium.

The insertion of hydrogen into the palladium produces no excess power.

The excess power in the deuterium - palladium case varies with time.

The magnitude of the excess power depends upon the value of the supplied power.

The Question of Chemical Reactions

If some kind of chemical reaction were responsible for the observed difference in behavior between the hydrogen and deuterium cases, we have to explain the following:

What is so different about hydrogen and deuterium chemistry ?

Why is there a time - dependence to the deuterium data, but not to the hydrogen data ?

Why does the effect increase with time in the deuterium case, rather than decrease with time, as we would expect if some chemical reactant were being consumed ?

Residual Questions

Is the internal heat generation related to the imposed interfacial potential (e.g. the Fermi level of the electrons in the PdD_x) or to the applied current ?

Is this a bulk or a surface effect ?

Is the presence of lithium important ?

Is the identity of the lithium isotope important ?

Is there a real volume effect ?

Is there a surface area effect ?

Is there an effect of surface shape - e.g. protrusions ?

Why is this not a steady state phenomenon ?

What is the real characteristic time of the phenomenon ?

What is the mechanism causing the internal heat generation ?

Richard L. Garwin
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(914) 945-2555

July 17, 1989
(Via FAX to 9-(409)-845-1899)

Professor Kevin L. Wolf
Cyclotron Institute
Texas A&M University
College Station, TX 77843

Dear Professor Wolf:

I write to invite further information about your findings of tritium production in electrolytic cells.

As you will remember, I talked to you in May, just before I briefed the National Science Board in Washington, and I have followed with interest your results reported at the Santa Fe workshop, and since then. I have been particularly interested in the recent information communicated by John Bockris that you have catalyzed the recombination of hydrogen gas from the electrolysis, and you have found a further increase by a factor hundred or thousand over the amount of tritium that had been measured in the electrolytic cell.

My comments now are to draw some conclusions from these measurements and to ask for readily available evidence that you might obtain from the cells that have been producing tritium.

First, if the fusion reaction has the usual 50:50 branching ratio, then each T produced must have been accompanied by a neutron. The mean life of T is 10**7 minutes, so if one has 10**6 dpm/ml, this corresponds to something like 10**13 T present per ml. If the cell has 10 ml of volume, then you must have produced 10**14 atoms of T. If this was done in 10 hours or 4 x 10**4 sec, the production rate was about 2 x 10**9 per sec. Thus, the question is whether neutrons were produced at the rate of 2 x 10**9 per sec or not.

Of course, such a neutron production rate would have given a dose of 1 Rem per hour at a distance of 1 m, and this could not have been missed. Furthermore, such a large number of neutrons would have produced radioactivity in various of the surroundings, which could be looked for now. So, my question is what measurements you have made at the time or could make now to set an upper limit on the number of neutrons produced to accompany the 10**14 T that the measurements of T activity in the LIQUID indicate.

Although Professor Bockris's letter was not clear on this point, if I assume that the increase of T by recombining the gas refers to an increase above this large activity found in the electrolyte, then a mere factor 40 increase would mean a neutron production rate of 40 x 2 x 10**9 per sec, or some 10**11 neutrons produced per second for the 10 hours or so required to produce the T. This would be a lethal dose at a distance of 1 m, and would, of course, be terribly obvious on any health physics monitor, film badge, or the like.

So that is the first question: If the cold fusion that produces T has the 50:50 branching ratio, where are the neutrons of the accompanying branch?

Although the proton energy of 3 MeV exceeds the deuteron binding energy of 2.2 MeV, the p+d>p+p+n reaction on the Pd-D will not go; the energy in the p+d center of mass is only 2/3, or 2.0 MeV.

Second question: If the cold fusion producing T has NO accompanying He-3 branch, there will be gamma rays produced by the 3-MeV protons themselves, by Coulomb excitation and proton bremstrahlung on Pd. According to the David Bailey paper of 04/20/89, these should amount to aout 10**-7 gammas per proton, in the readily detectable lines between 373 keV and 556 keV. If you are making 10**11 T/s, have you looked for the 10**4 gamma/s?

Could you please reply by FAX to tell me what measurements have been made of the neutron production rate accompanying these electrolytically produced tritium concentrations? And the gamma rate? If there were no contemporaneous measurements, can you tell me what is the ambient radioactivity in the materials or the neighborhood of the cells. In fact, you might want to calibrate this induced radioactivity with certified neutrons produced by a Cf source or a Pu-Be source.

I am most concerned to know whether there is any consistent number of neutrons or gammas accompanying the T that you believe has been produced from the electrolytic cells.

Thank you very much.

Sincerely yours,

Richard L. Garwin

cc:
W.L. Woodard, DOE (Via FAX to 9-(202)-586-3119).

RLG:jtml:198%KLW:071789.KLW

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July 17, 1989
(Via FAX to 9-011-(41-22) 783-0600)

Dr. Douglas R.O. Morrison
CERN Laboratoire 1
1211 Geneve 23
SWITZERLAND

Dear Douglas,

Enclosed, as you requested, is the report of our Panel.

I hope you find this of interest. Futhermore, there has been some excellent work done by the folks at Sandia, of which I send you the first page. These folks conclude with the statement "It should be STRESSED that without neutron counter redundancy, reports of the irreproducible generation of neutrons, including bursts, should be treated with great skepticism."

If you talk with these people (which I have not yet), please give them my regards.

On 07/12/89, I sent Howard Menlove a FAX, which I enclose. I will call him now, because I have not heard from him.

Very best regards.

Incidentally, Doug, I don't have your office and home numbers handy. Would you please include them in your next communication to me? Thanks very much.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

Encl:
- 07/12/89 Draft Interim Report of the Cold Fusion Panel to the Energy Research Advisory Board. (071289.WLW)
- 06/28/89 "Search for Cold Fusion in High Pressure D2 Loaded Ti and Pd Metal and Hydride," by J.E. Schirber, et al, Sandia National Laboratories (page 1 only). (062889.MAB)
- 07/12/89 LTR RLG to H.O. Menlove re "Measurement of Neutron Emission ..."

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18 Jul 89 23:26:22 GVA
Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Thu, 07
89 23:26:06 GVA
Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

id AA12310; Tue, 18 Jul 89 23:25:36 +0200
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id AA05346; Tue, 18 Jul 89 23:22:51 +0200
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Subject: █
To: CBOWMAN@LAMPF, JONESSE@BYUVAX, BOND@BNLDAG, ESCOBAR@FNAL, PIRES@BRUSP,
gai@yalevm, rlg2@yktvmv, sgrard@bmsueml1
X-Vms-To: @LIST7.DIS

Dear E632 and WA84 Collaborators, 16 July 1989.

LEP NEWS

COLD FUSION NOTES No. 17

1. Introduction

2. LEP Start up

3. United States DOE Panel gives Interim Report
    3.1 General
    3.2 Calorimetry
    3.3 Fusion Products
    3.4 A Comment from Utah

4. Funding in Utah

5. More News of Tritium Discoveries in Texas - Editorial in Nature

6. Other Items
    6.1 Situation in Utah
    6.2 Brazil
    6.3 Search for Neutrons at Sandia
    6.4 Bursts of Neutrons at Los Alamos

1. INTRODUCTION

After many years work, the Large Electron-Positron Collider, LEP at CERN, started its tests on the 14 July as scheduled and a positron beam was able to circulate round the entire 27 km ring.

The panel set up by DOE has issued an interim report. Essentially it said that there was no good evidence that cold fusion could produce a useful source of energy and hence there should be no major funding for it. Some things were not understood and a modest amount of financing would be acceptable - but on the basis of competitive proposals.

2. LEP START UP

On Friday, July 14, positrons were injected for the first time into the LEP storage ring. This ring is about 27 km in circumference and is about 100 metres underground. The injection from the SPS accelerator, began at 15.30 and went round to the first luminescent screen and was seen and the screen was lowered, then to the second screen where it was seen and that screen was lowered and so on. There was so little difficulty that by 16.30 the positrons were able to go all the way round! Considering the number of components and the precision required, it is a remarkable achievement. By now 16 turns have been achieved. The beam is about a few times ten to the nineth.

The aim is to accelerate simultaneously intense electron and positron beams to about 50 GeV and make them collide to produce Z-zero particles. SLAC has just given its first physics result from its electron-positron collider, that the mass of the Z0 is 91.2 +/- 0.4 GeV - this is almost an order of magnitude more precise than the previous value.

At present the LEP schedule is to continue commissioning until early August when there will be a three day test run of the four LEP experiments, then more commissioning until September when it is hoped to start full physics runs.

(By 17 July improvements have been made to the SPS and 30 turns have been made).

3 UNITED STATES DOE PANEL GIVES INTERIM REPORT.

3.1 General

The US Department of Energy had asked a 22-member committee to report on Cold Fusion. The Co-chairs are a nuclear chemist, John R. Huizenga and a physicist, Norman Ramsey. They have issued a preliminary report (ahead of their own schedule) on July 12 (sorry for the delay but I finally took a day off to ski on the Mont Fort glacier above Verbier, also we are starting another test run of our WA84 scintillating fibres). The report said;

"the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion".

"evidence for the discovery of a new nuclear process termed cold fusion is not presuasive"

"no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time."

The panel distinguished between the excess heat ["Fleischmann-Pons effect", my comments are in brackets] claiming production of relatively large amounts of heat(some Watts), and the reports of production of small amounts of fusion products or of very small amounts of heat (it was recalled that one Watt corresponded to about 1 E12 neutrons).

The panel said that to clear up conflicting results and for general scientific interest, some experiments should continue, but suggested three conditions; (1) the funding be modest, (2) it should be on the basis of competitive proposals [which implies that you have to explain what you have done and what you intend to do] (3) co-operation between labs is encouraged, (4) for calorimetry, the use of closed calorimeters is encouraged

3.2 Calorimetry

The panel wrote that Calorimetry was difficult and its analysis complicated. They recommended that closed cells be used where there were fewer uncertainties rather than open cells (where the D2 and O2 gases escape) [claims of excess heat have only come from open cells]. "In none of our visits to the different sites did we see an operating cell that was actually producing excess heat" [the sites included Utah and Texas A&M].

"However there are reports of sporadic temperature 'excursions' or 'bursts' that apparently represent power outputs significantly larger than the input power. These.....are not at present understood".

The panel recommended some research to "resolve some of the claims and counter claims in calorimetry .....supported at a modest level" of funding, but "against any significant expenditure".

3.3 Fusion products

Neutrons - "Although many experimenters report no neutrons, some report as many as one neutron per second". As this is 1 E-12 Watts "it is of no interest as a practical energy source".

Tritium - "Numerous experimenters"... "have found no excess tritium". "one group reports finding up to 1 E14 tritium atoms". In similar cells they find about one neutron per second; this is about 1 E10 less than expected.

Helium - "None of the researchers to date ...have reported 3He or 4He above the detectable level of 1 E9 atoms".

Volcanoes - Some groups are looking for tritium from volcanoes as it has been suggested cold fusion could produce this.

The panel recommended (1) some experiments to check claims of excess tritium (2) some studies of other fusion products for reasons "of general scientific interest" (3) when experiments searching for excess heat are being performed, some simultaneous monitoring for fusion products should be done. All these experiments should be modest in funding.

3.4 A Comment from Utah

The New York Times followed its excellent tradition of not just copying press releases, but of contacting other sources to obtain balancing comments. Thus they quote "In a telephone interview, Dr. James Brophy, director of research at the University of Utah, dismissed the panel's findings, saying he was confident cold fusion was real."

4. FUNDING IN UTAH

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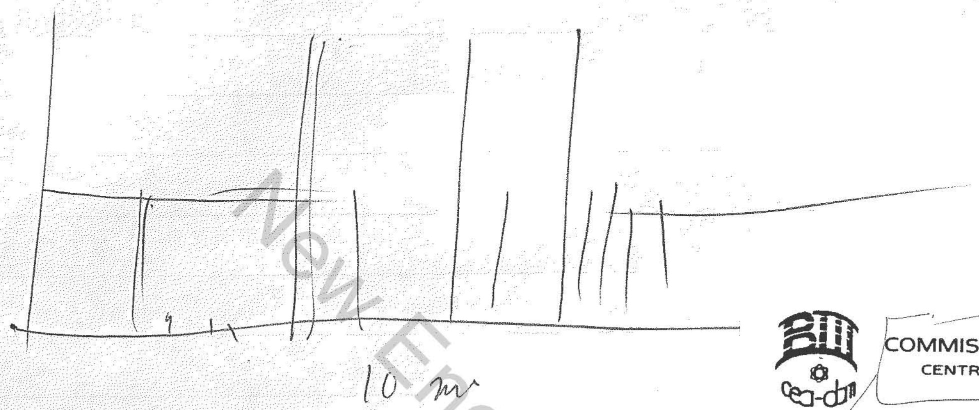
Douglas R. O. Morrison.

07/15/91

Fresti



COMMISSARIAT A L'ÉNERGIE ATOMIQUE
CENTRE D'ÉTUDES DE BRUYÈRES-LE-CHÂTEL

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(33-1)

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e-mail "vmcp at frsac11. bitnet"

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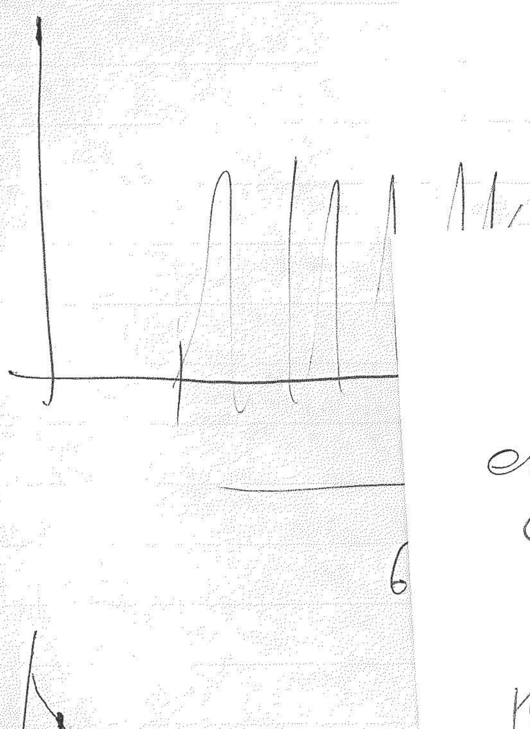

Babuel - Peyrissac

Monsieur Jean-Paul Babuel-Peyrissac
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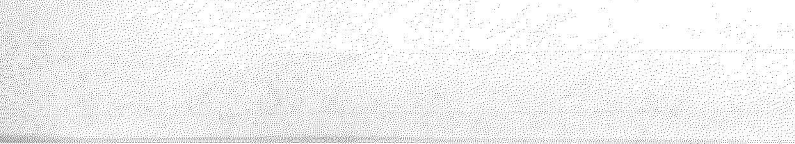
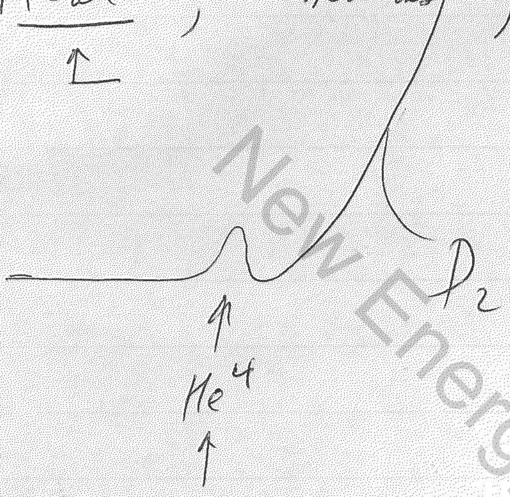
75015
FRANCE



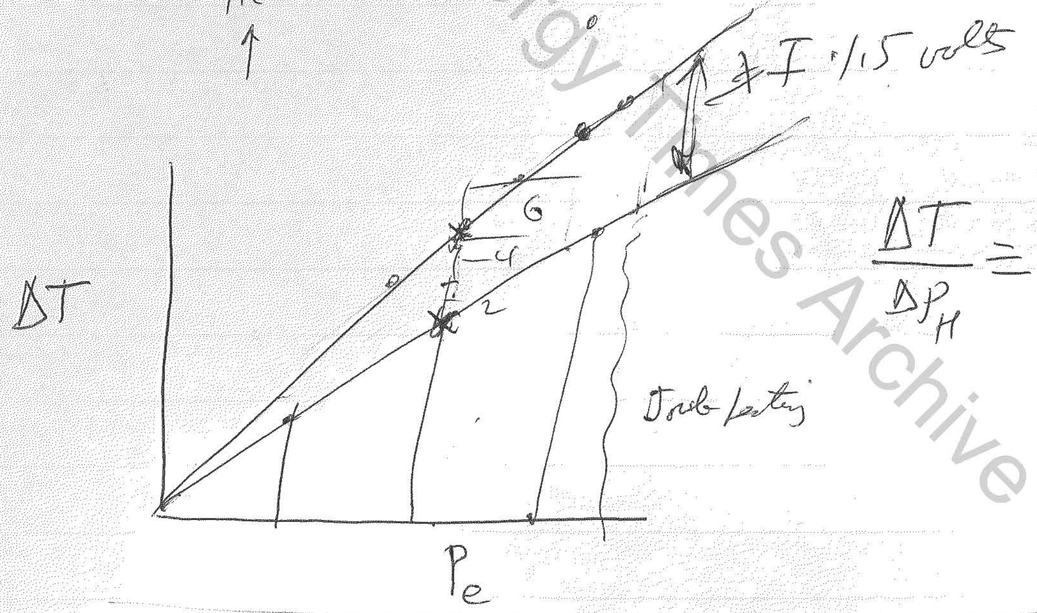
07/15/89

Etienne Roth
Claude A. Philie
Jean-Paul Bobuel-Peyresse

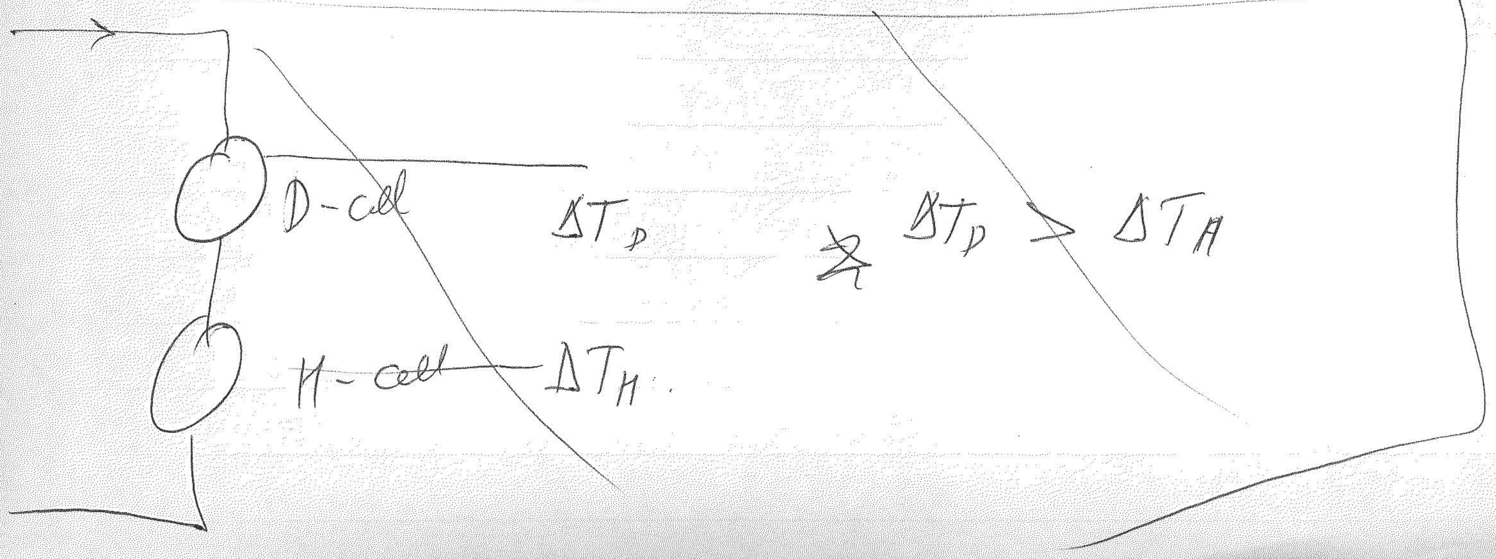
Heat , neutrons , T , He^3 , He^4.



Charles Walling



$$\frac{\Delta T}{\Delta P_H} \cong K \sim \underline{0.6°/W}$$



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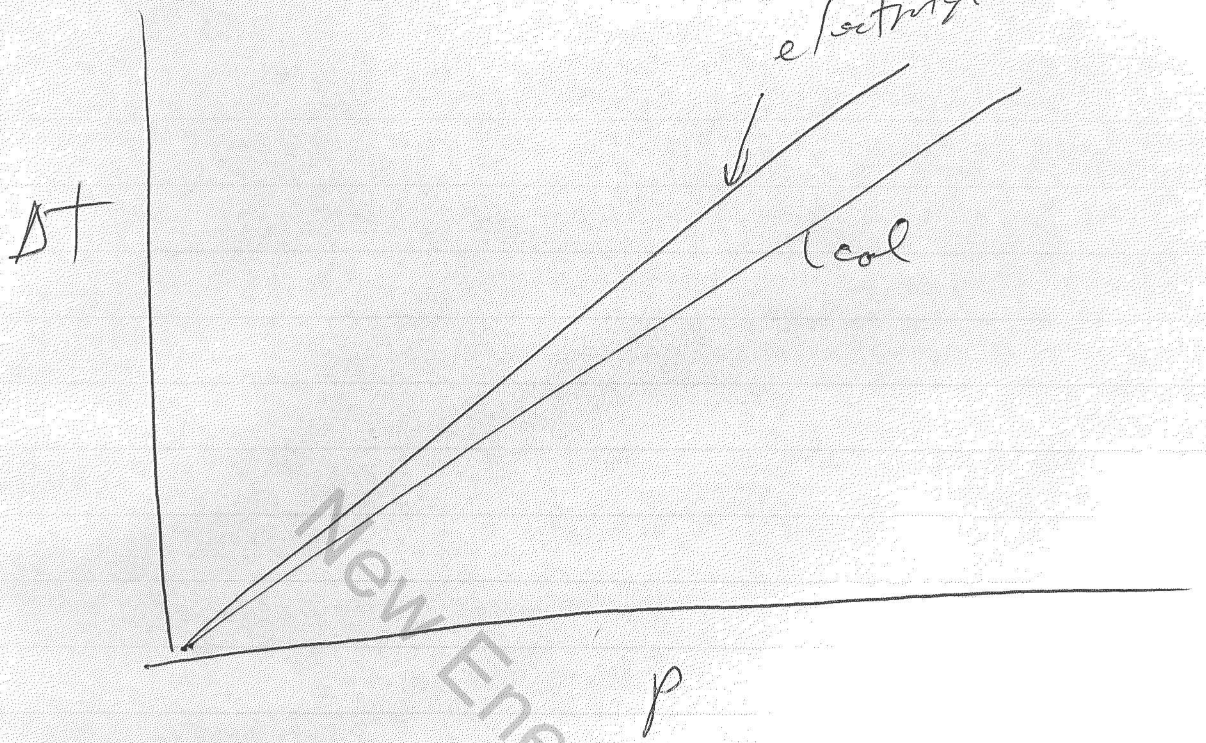
ΔT
electrolytic
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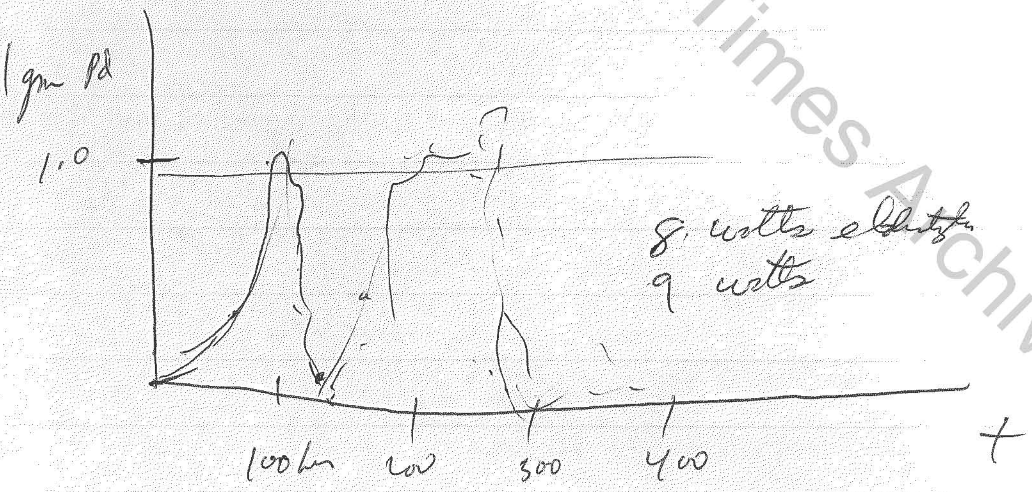
p


W/gm Pd
1.0
8. watts electrolytic
9 watts
100 hrs
200
300
400
t

07/15/89

⑤

$\boxed{3\,\text{MeV} + 0.92\,\text{MeV}}$

$D+D \rightarrow p + H^3$

$\rightarrow n + He^3$ (D-D-H)

10^6 dpm/ml $\rightarrow$ 10^7 dpm

$\lambda^{-1} = \left[12.\times\frac{1}{0.697}\right] = 18\times 3\times10^7$

$= 5\times10^8$ sec

10^7 [illegible]

10^{14} T ✓

10 hours $= 4\times10^4$ sec

$\dot{T} = 2\times10^9\ \text{sec}^{-1} = \boxed{\dot{n} = 2\times10^9/\text{sec}}$

×40

$\boxed{10^{11}\ \text{sec}}$

$\boxed{20\ \text{mg/cm}^2}$

$\frac{5\ \text{mg/cm}^2\ \text{of } [illegible]}{160\ \text{g/cm}^2}$

$p+D \rightarrow p+p+n$

$\rightarrow \frac{1}{3}\times10^{-4} \rightarrow$ ~~10^7 n/sec~~ 3×10^6 n/sec

07/15/89

④

$10^{-24}\ \text{cm}^2$

$\cancel{D}\ p + \boxed{Pd + D}$

110 amu

$$\cancel{K} \quad n\sigma = \cancel{A}\ \frac{10^{24}}{6\times10^{23}} \times 110$$

$$= 170\ \text{g/cm}^2$$

Call Kevin Wolfe -- knock-on neutron flux

Document burst

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--------------------------------------------------------------------
 Received: from CEARN by CEARN.cern.ch (Mailer R2.03B) with BSMTP id 0580; Tue,
  18 Jul 89 23:26:22 GVA
 Received: from mint.cern by CEARN.cern.ch (IBM VM SMTP R1.2.1) with TCP; Thu, 07
   89 23:26:06 GVA
 Received: from cernvax.uucp (cernvax) by mint.cern (cernvax) (4.12/3.14)

id AA12310; Tue, 18 Jul 89 23:25:36 +0200
 Received: by cernvax.uucp (5.57/Ultrix2.0-B)

id AA05346; Tue, 18 Jul 89 23:22:51 +0200
 Message-Id: <8907182122.AA05346@cernvax.uucp>
 Date: Tue, 18 Jul 89 23:20 GMT +1
 From: MORRISON%VXPRIX.decnet.cern.ch@cernvax
 Subject: [redacted]
 To: CBOWMAN@LAMPF, JONESSE@BYUVAX, BOND@BNLDAG, ESCOBAR@FNAL, PIRES@BRUSP,
        gai@yalevm, rlg2@yktvmv, sgrard@bmsueml1
 X-Vms-To: @LIST7.DIS
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Dear E632 and WA84 Collaborators,   16 July 1989.

LEP NEWS

COLD FUSION NOTES No. 17

1. Introduction

2. LEP Start up

3. United States DOE Panel gives Interim Report
    3.1 General
    3.2 Calorimetry
    3.3 Fusion Products
    3.4 A Comment from Utah

4. Funding in Utah

5. More News of Tritium Discoveries in Texas - Editorial in Nature

6. Other Items
    6.1 Situation in Utah
    6.2 Brazil
    6.3 Search for Neutrons at Sandia
    6.4 Bursts of Neutrons at Los Alamos

1. INTRODUCTION

After many years work, the Large Electron-Positron Collider, LEP at CERN, started its tests on the 14 July as scheduled and a positron beam was able to circulate round the entire 27 km ring.

The panel set up by DOE has issued an interim report. Essentially it said that there was no good evidence that cold fusion could produce a useful source of energy and hence there should be no major funding for it. Some things were not understood and a modest amount of financing would be acceptable - but on the basis of competitive proposals.

2. LEP START UP

On Friday, July 14, positrons were injected for the first time into the LEP storage ring. This ring is about 27 km in circumference and is about 100 metres underground. The injection from the SPS accelerator, began at 15.30 and went round to the first luminescent screen and was seen and the screen was lowered, then to the second screen where it was seen and that screen was lowered and so on. There was so little difficulty that by 16.30 the positrons were able to go all the way round! Considering the number of components and the precision required, it is a remarkable achievement. By now 16 turns have been achieved. The beam is about a few times ten to the nineth.

The aim is to accelerate simultaneously intense electron and positron beams to about 50 GeV and make them collide to produce Z-zero particles. SLAC has just given its first physics result from its electron-positron collider, that the mass of the Z0 is 91.2 +/- 0.4 GeV - this is almost an order of magnitude more precise than the previous value.

At present the LEP schedule is to continue commissioning until early August when there will be a three day test run of the four LEP experiments, then more commissioning until September when it is hoped to start full physics runs.

(By 17 July improvements have been made to the SPS and 30 turns have been made).

3 UNITED STATES DOE PANEL GIVES INTERIM REPORT.

3.1 General

The US Department of Energy had asked a 22-member committee to report on Cold Fusion. The Co-chairs are a nuclear chemist, John R. Huizenga and a physicist, Norman Ramsey. They have issued a preliminary report (ahead of their own schedule) on July 12 (sorry for the delay but I finally took a day off to ski on the Mont Fort glacier above Verbier, also we are starting another test run of our WA84 scintillating fibres). The report said;

"the experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion".

"evidence for the discovery of a new nuclear process termed cold fusion is not presuasive"

"no special programs to establish cold fusion research centers or to support new efforts to find cold fusion are justified at the present time."

The panel distinguished between the excess heat ["Fleischmann-Pons effect", my comments are in brackets] claiming production of relatively large amounts of heat(some Watts), and the reports of production of small amounts of fusion products or of very small amounts of heat (it was recalled that one Watt corresponded to about 1 E12 neutrons).

The panel said that to clear up conflicting results and for general scientific interest, some experiments should continue, but suggested three conditions; (1) the funding be modest, (2) it should be on the basis of competitive proposals [which implies that you have to explain what you have done and what you intend to do] (3) co-operation between labs is encouraged, (4) for calorimetry, the use of closed calorimeters is encouraged

3.2 Calorimetry

The panel wrote that Calorimetry was difficult and its analysis complicated. They recommended that closed cells be used where there were fewer uncertainties rather than open cells (where the D2 and O2 gases escape) [claims of excess heat have only come from open cells]. "In none of our visits to the different sites did we see an operating cell that was actually producing excess heat" [the sites included Utah and Texas A&M].

"However there are reports of sporadic temperature 'excursions' or 'bursts' that apparently represent power outputs significantly larger than the input power. These.....are not at present understood".

The panel recommended some research to "resolve some of the claims and counter claims in calorimetry .....supported at a modest level" of funding, but "against any significant expenditure".

3.3 Fusion products

Neutrons - "Although many experimenters report no neutrons, some report as many as one neutron per second". As this is 1 E-12 Watts "it is of no interest as a practical energy source".

Tritium - "Numerous experimenters"... "have found no excess tritium". "one group reports finding up to 1 E14 tritium atoms". In similar cells they find about one neutron per second; this is about 1 E10 less than expected.

Helium - "None of the researchers to date ...have reported 3He or 4He above the detectable level of 1 E9 atoms".

Volcanoes - Some groups are looking for tritium from volcanoes as it has been suggested cold fusion could produce this.

The panel recommended (1) some experiments to check claims of excess tritium (2) some studies of other fusion products for reasons "of general scientific interest" (3) when experiments searching for excess heat are being performed, some simultaneous monitoring for fusion products should be done. All these experiments should be modest in funding.

3.4 A Comment from Utah

The New York Times followed its excellent tradition of not just copying press releases, but of contacting other sources to obtain balancing comments. Thus they quote "In a telephone interview, Dr. James Brophy, director of research at the University of Utah, dismissed the panel's findings, saying he was confident cold fusion was real."

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explanations can be
, it stands, the result
refutes our hypothesis.
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ng, but has other
ons besides that of an

rk is rife with pitfalls[7],
of previously uncon-
or other factors con-
s is always present. The
ation between parasite
howiness still appears
allenges are to test the
pecies[8] and to elucidate
meaning of sexually
s to animals in their
nts.

MARLENE ZUK

logy,
Mexico,

1, USA

EY REPLY—Zuk claims
ve data we used to test
ry of sexual selection[2]
nt correlations between
d parasite prevalence in
That claim is wrong
on cross-species corre-
stablished that the non-
pecies points means that
s attached to cross-
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ociations have been
lled for[3,7,9–11]. The non-
tions presented in our
using analyses which

males will obtain a greater mating advantage in brighter species, which will consequently harbour a lower parasite load. We know of no data which bear on the issue of whether female choice increases resistance in the host population. In the meantime, therefore, it seems prudent to consider the comparative prediction as two-tailed.

Zuk questions our reasons for re-scoring the showiness of European birds[3,6]. As we stated[1], our failure to replicate the patterns found using Hamilton and Zuk's[2] scores of

1. Read, A.F. & Harvey, P.H. *Nature* **339**, 618–620 (1989).
2. Hamilton, W.D. & Zuk, M. *Science* **218**, 384–387 (1982).
3. Read, A.F. *Nature* **327**, 68–70 (1987).
4. Read, A.F. *Trends Ecol. Evol.* **3**, 97–102 (1988).
5. Endler, J.A. & Lyles, A.M. *Trends Ecol. Evol* (in the press).
6. Baker, R.R. & Parker, G.A. *Phil. Trans. R. Soc.* B**267**, 63–130 (1979).
7. Pagel, M.D. & Harvey, P.H. *Q. Rev. Biol.* **63**, 413–440 (1988).
8. Zuk, M., Thornhill, R., Ligon, J.D. & Johnson, K. *Am. Zool.* (in the press).
9. Harvey, P.H. & Mace, G. in *Current Problems in Sociobiology* (ed. King's College Sociobiology Group) 343–362 (Cambridge Univ. Press, 1982).
10. Ridley, M. *The Explanation of Organic Diversity* (Clarendon, Oxford, 1983).
11. Felsenstein, J. *Am. Nat.* **125**, 1–15 (1985).

Cold fusion ideas

SIR—In the excitement about the possibility of chemically induced nuclear fusion[1,2], there is a danger that observations may be considered indicative of fusion when more conventional explanations from cognate fields are overlooked[3]. We wish to draw attention to two such cases.

First, consider an experiment in which identical currents are passed through identical electrolytic cells containing electrolytes differing only in the substitution of heavy water (D_2O) for H_2O, and the temperature of each cell is monitored. It has long been established that the viscosity of D_2O is appreciably higher than that of H_2O (1.26×10^{-4} Pa s and 1.009×10^{-4} Pa s at 20 °C, respectively)[4]. Making the assumption that H_2O/D_2O systems obey Walden's rule[5], the resistivity of a salt solution in D_2O will be 25 per cent higher than the corresponding solution in H_2O; for a 0.1 M lithium acetate electrolyte solution, running at a current density of ~0.1 A cm^{-2} (ref. 2) between electrodes 1 cm apart, this will correspond to an excess heat generation in the D_2O solution relative to the H_2O solution of ~1.4 W per cm^2 of electrode surface. The effect should be still more marked with a strong acid or strong alkali (for example, LiOD) as electrolyte, where the conduction mechanism involves H/D tunnelling. We have checked that heat differentials of this order are in fact observed; we find that such cells typically run with D_2O 2–3 °C hotter than H_2O, with an overall temperature rise of 10 °C above ambient.

As a second example, tritium atoms should be produced in the proposed fusion reactions[1,2], and scintillation measurements of tritium decay have been reported[2]. There have been suggestions that tritium has been detected mass spectrometrically as the ion DT^+ (with a mass/charge ratio of 5 a.m.u.). In such an experiment the gas that diffuses through the walls of a closed palladium tube, which forms the cathode of an electrolytic cell, is observed using a mass spectrometer. We have duplicated this experiment, and find that there are signals both at mass 5 and at mass 6; both are readily detected using a quadrupole mass spectrometer when the D_2 pressure in the system exceeds 10^{-9} mbar. These ions can also be generated, however, by admitting D_2 to the system. Ions of mass 5 and 6 are isotopically substituted H_3^+ ions, D_2H^+ and D_3^+; H_2 is always present as a background gas. The H_3^+ ion has a history as old as mass spectrometry itself[6,7] and is well recognized as the product of reactions inside the mass spectrometer[8], although the analysis has been carried out at pressures higher than those used here. The intensities of the various H_3^+ ions have nonlinear dependencies on pressure, and because the pressure increases in an electrolysis experiment, the concomitant intensity changes could be misinterpreted as the result of a time-dependent process.

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D. R. LLOYD

Department of Chemistry,

J. M. D. COEY

Department of Physics,
Trinity College,
Dublin 2, Ireland

[1] Jones, S.E. *et al.* *Nature* **338**, 737–740 (1989).

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those with a prolonged rep
delay, such as the Europea
(*Meles meles*), which normally
pause of 10 months, or the
(*Gulo gulo*), which has a diapa
months[2]? We urge anyone wh
newly caught wild mammals tc
this phenomenon.

ELEANOR
H. P.
Florida Institute of Technology,
Melbourne, Florida 32901, US

1. Storrs, E.E., Burchfield, H.P. & Rees, R.J.W
59, 11–15 (1988).
2. Mead, R.A. in *Embryonic Diapause in Mam*
Fert. Suppl. **29**, 11–24 (1981).

Cell identity resolved

SIR—Stratton *et al.* recently po
that the putative medulloblas
line TE671 "is most probably a
the rhabdomyosarcoma cell li
Having carried out a cytogenetic
support that view.

The TE671 cell line (ATCC
came to the American Type
Collection (ATCC) in 1982[2]
Naval Biosciences Laborator

NATURE

Richard L. Garwin
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(914) 945-2555

July 12, 1989
(Via FAX to 9-(505)-667-7626)

Dr. Howard O. Menlove
Los Alamos National Laboratory
P.O. Box 1663, MS E-540
Los Alamos, NM 87545

Dear Howard,

You have probably received the message that I called from the ERAB Cold Nuclear Fusion Advisory Panel meeting in Washington 07/11/89. Here is what I wanted to ask, and I would be grateful for your reply by FAX, if possible.

Referring to your "Measurement of Neutron Emission ..." submitted to Nature 06/12/89, that we had at our Advisory Panel meeting in Washington 06/22/89 (LA-UR 89-1974), we find on page 3.9 (9/10 of the way down page 3) "The five ... pods on the circumference of System 3 can be used to determine the average neutron energy when sufficiently high neutron yields are obtained."

You may recall that I emphasized that the neutron "bursts" from Frascati could hardly be believed unless they produced counts simultaneously in a similar counter similarly disposed with respect to the source, whenever they produced massive numbers of counts in one counter. I urged also that LANL separate the counters in one of the detectors in order to ensure that electronically independent systems registered at the same time, coupled only by neutrons.

On reading further in your LA-UR 89-1974, I find on page 10.6, "We have not yet measured the neutron energies because the yields still are too low for the neutron spectrometry measurements." But I see in your Figure 4 149 counts on 05/21/89 in "System 3." What is the probability that 149 counts would be obtained in the inner counter tubes and none in the five outer tubes? In fact, if you have a record that the five outer tubes did not count in that 128 musec, this casts grave doubts on the identification of the burst as neutrons.

So please tell me whether you have made runs in which the counts of the outer five counters in System 3 can be correlated in time with the counts of the inner counters.

This is of particular importance because I have just read the Sandia paper "Search for Cold Fusion in High Pressure ...", by J.E. Schirber, et al, and I find on page 5.4 a very interesting paragraph,

> "We should point out however, that apparent neutron bursts have been observed a number of times in single counter channels ... Only those events which are observed simultaneously in our three counters with the correct relative intensities can be considered as arising from real bursts."

And on page 5.9,

> "It should be stressed that without n-counter redundancy, reports of the irreproducible generation of neutrons, including bursts, should be treated with great skepticism."

So now I have to ask for your data showing simultaneous counts in redundant counters.

Furthermore, I have to ask for your data showing counts in one counter set and no counts in the counters that might be expected to be influenced by the neutron bursts. These data should be presented, whether they fit your interpretation or not.

Finally, on re-reading your LA-UR 89-1974, I note that it had no experiments with pure hydrogen on metal, but only a 50:50 mixture. I don't recall seeing in the weekly Update from LANL any pure-hydrogen experiments. Have you performed them? What are the data?

Please reply to me by FAX to (914) 945-2141 with a copy to Bill Woodard at (202) 586-3119.

Thanks very much.

Sincerely yours,

Richard L. Garwin

cc:
W.L. Woodard, DOE (Via FAX to 9-(202)-586-3119).

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